AMERICAN JOURNAL of PHYSICS

A Journal Devoted to the Instructional and Cultural Aspects of Physical Science

VOLUME 13, NUMBER 4 AUGUST, 1945

Molecular Structure and Mechanical Properties of High Polymers—A Review

H. MARK

Polytechnic Institute of Brooklyn, Brooklyn, New York

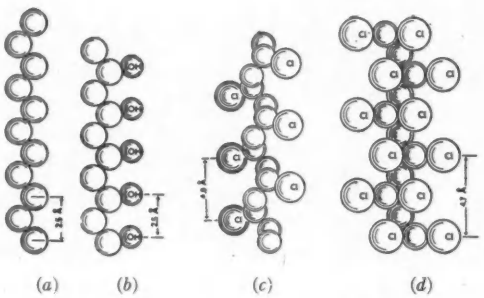
FOR many centuries man has used natural products such as wood, stone, cotton or wool, and manufactured inorganic systems, such as ceramics, glass or metals, to make houses, clothing, machines and most other necessities and luxuries. Within the last few decades another new type of building material has been discovered and developed, namely, the *synthetic organic polymers*. They seem to be of considerable and ever-increasing interest and importance for large branches of industry and are particularly apt to provide for great improvements in the construction of houses, ships, vehicles, airplanes, electric engines, scientific instruments, tires, shoes and clothing of all kinds. Some of them, such as Nylon, Cellophane, Bakelite, buna rubber, rayon and celluloid are well known, even to the general public, because of their outstanding properties; but many more of them are being developed in the industrial research laboratories and will soon be ready for use by consumers. Common to all of them is the fact that they are of preponderantly organic nature, which means that they consist mainly of carbon, hydrogen, oxygen and nitrogen, although some of them, such as Vinylite, Pliolite and Saran, contain also chlorine, and others, such as the silicones, contain a certain amount of silicon. Another important fact is that most of these organic *polymers*, or *high polymers*, can be produced from such simple and comparatively abundant raw materials as air, water, coal, oil, limestone, rocksalt and sand with the aid of large-scale chemical operations, which eventually will make the products reasonably cheap.

The term *high polymer* is a generic term and refers to a material composed of molecules built on peculiar principles. First, the word "high" indicates that they all consist of molecules of very high molecular weight. It will be remembered that the molecular weight of hydrogen is 2; of oxygen, 32; and of sugar, 342. The molecular weight of an organic high polymer (synthetic or natural) is of the order of magnitude of ten thousand to several millions. Table I contains a few representative figures to illustrate this point. It can be seen that all materials listed in the table, whether they are natural, such as cellulose in cotton, protein in silk and rubber in hevea

TABLE I. Molecular weights and degrees of polymerization of some representative polymers.

Material	Elements	Molecular weight	Degree of polymerization
Cellulose in native cotton	C, H, O	≤1,600,000	≤10,000
Cellulose in wood pulp	C, H, O	*ca.* 500,000	*ca.* 3000
Cellulose in rayon	C, H, O	80,000–160,000	500–1000
Cellulose acetate in rayon	C, H, O	80,000–140,000	300– 500
Cellulose acetate in film	C, H, O	100,000–160,000	400– 600
Native rubber	C, H	200,000–400,000	3000–6000
Buna-S	C, H	100,000–150,000	1500–2000
Polyvinyl chloride (Geon)	C, H, Cl	100,000–200,000	1500–3000
Polyvinylidene chloride (Saran)	C, H, Cl	100,000–300,000	1000–3000
Polyvinyl chloride acetate (Vinylite)	C, H, O, Cl	100,000–200,000	1500–3000
Polystyrene	C, H	100,000–300,000	1000–3000
Polymethylmethacrylate (Lucite)	C, H, O	100,000–200,000	800–1600
Protein in native silk	C, H, O, N	*ca.* 150,000	2500
Polyamides (Nylon)	C, H, O, N	*ca.* 25,000	250

BOSTON UNIVERSITY
COLLEGE OF LIBERAL ARTS
LIBRARY



(a) (b) (c) (d)

FIG. 1. Structural details of the molecular chains of some polymers. (*a*) polyethylene; (*b*) polyvinyl alcohol; (*c*) polyvinyl chloride; (*d*) polyvinylidene chloride.

latex, or synthetic, such as Nylon, Vinylite, polythene, Saran and Vistanex, have molecular weights in the tens or hundreds of thousands.

Second, the word "polymer" itself consists of two Greek words *polys*, which means "many," and *meros*, which means "part." It indicates that the molecules of all these substances are built up from many equal parts by the repetition of one and the same fundamental unit. This unit, which is called the *monomer*, is the isoprene residue in the case of rubber, the glucose residue in the case of cellulose, isobutylene in the case of Vistanex, phenol and formaldehyde in the case of Bakelite, and so on. A high polymer is therefore a substance having very large molecules, each of which is built up by the repetition of one (or several) fundamental units (monomers). If one divides the molecular weight of the polymer by the molecular weight of the monomer, the result is the number of consecutive repeating units. This is called the *degree of polymerization*, or *DP*, of the material under consideration. Table I includes the *DP* for the substances listed; it is evident that all are of the order of magnitude of a thousand.

A third characteristic feature is that in all synthetic organic polymers the monomer units are linked to each other by *chemical forces* to form the giant molecule of the polymer. These forces are large and lead to homopolar bonds, the dissociation energies of which are between 70 and 100 kcal/mole. Such bonds can be severed only by attack with very reactive chemicals or by the use of temperatures above 250° or 300°C; they remain essentially unaffected by dissolving, melting, or by such thermo-mechanical treatments as spinning, casting, extruding, injecting or molding. Finally, it may be added that in many, though not in all, synthetic organic polymers the monomer units are linked together in such a way that they form long chainlike molecules which, because of the rotational mobility of most homopolar bonds, possess a certain degree of internal flexibility.

To summarize: organic high polymers consist mostly of long and flexible chain molecules of high molecular weights, in which the monomers are linked together by strong chemical bonds. Figure 1 shows a few examples of such chain molecules according to the x-ray analysis of a number of polymers[1] in the crystalline state.

After these brief general remarks about the common structural principles of organic polymers, we can ask the question: Do we know what connections exist between the principle structural features of a given polymer and the mechanical properties of samples made from it? Such knowledge, if it exists, obviously would be of great value in designing polymers for special purposes and in improving their properties. A fair answer to this question seems to be that we do not have a well-developed mathematical theory which connects quantitatively the structure of a given polymer with its properties, but we do know a number of general rules and principles which correlate properties and structure and which have been helpful in many cases in obtaining materials with superior properties.

There are two main factors that influence the ultimate behavior of a polymer: (i) the structural configuration of the long chain molecules themselves; and (ii) the way in which these molecules are arranged in the sample of the polymer. The same distinction can also be expressed in a somewhat briefer form in terms of (i) the *chemical structure* of the molecules and (ii) the *physical texture* of the macroscopic sample. We shall now attempt to discuss these two factors in order.

Details of Chain Structure and Mechanical Properties of Polymers

Let us first enumerate the most important structural details of high polymer molecules and briefly report our present knowledge on their significance for the mechanical properties of polymers. There are, first of all, the *average molecular weight* and the *average DP* of the substance, which, according to Table I, vary from 20,000 to 1,000,000 and from 100 to 5000, respectively. There is the *molecular weight distri-*

[1] See, for example, the excellent review by C. S. Fuller, Chem. Rev. **26**, 143 (1943).

bution, which describes the heterogeneity of the material and is comparatively narrow in some cases and fairly wide in others, and there is the internal *flexibility* of the individual chains, which is a consequence of the chemical nature of the bonds between the monomers.

The connection between mechanical properties and *average DP* has been frequently investigated.[2] The general result is that, in order to obtain mechanical strength at all, a certain minimum *DP* which ranges between 40 and 80 is necessary. As soon as this critical value is exceeded, the material starts to exhibit mechanical strength and its ultimate tensile strength increases from now on continually with the average *DP*. Figure 2 shows diagrammatically that up to a *DP* between 40 and 80 the tensile strength of a film cast from this material is negligible and that it then increases about in proportion to the chain length. The left-hand border of the shaded area, denoted by circles, holds for polyamides, while the right-hand border, denoted by crosses, holds for polyhydrocarbons. All other chain polymers, such as cellulose esters, polyvinyl derivatives, and so forth, lie in between. The proportionality between mechanical strength and *DP* holds up to a *DP* of about 250. Then the curve bends, and after a *DP* of about 600 is reached, the mechanical properties depend but slightly upon *DP*.

While the influence of the average *DP* on the mechanical strength is fairly well established, the significance of the molecular weight distribution is still an unsolved problem. Very promising attempts have been made recently[2] to establish a connection between the distribution curve and the mechanical properties of high polymers; it is found that relatively small amounts (between 10 and 15 percent by weight) of constituents having a *DP* below 150 are detrimental for such mechanical properties as tensile strength, resistance to flexing, fatigue performance, and so forth. Removal of such constituents of low molecular weight improves the material considerably.

Let us next consider the internal flexibility of the individual molecules. It is a well-established fact that each atom in a small molecule carries out rapid vibrations, the energy of which represents the main part of the heat content of a solid (crystallized) material and can be studied by the molecular spectrum (ultraviolet, infrared, Raman) and the temperature dependence of the thermal capacity. In larger molecules, such as the polymers listed in Table I, there exist in addition to these rapid vibrations of the individual atoms comparatively slow vibrational and rotational movements of certain parts or segments of a large molecule. This does not imply that the rotation about the single C−C bond is actually free. In fact, recent investigations have shown that, owing to the mutual interaction of the substituents, subsequent CH_2 groups in a long chain paraffin molecule cannot rotate freely about the connecting bond. However, vibrational movements within angles of about 10° are much less inhibited, and as a consequence a chain of many CH_2 groups—say a thousand or more—will exhibit *as a whole* a certain degree of internal flexibility. There are *no sharp bends* to be expected within the range of a few CH_2 groups, but the chain as a whole will be capable of assuming a large number of slightly curved or coiled geometric configurations, and the probability of finding such a large molecule in its most extended state is extremely small. If an isolated long chain molecule is brought into this state of extreme elongation by external forces (mechanical stretching), it has the tendency, under the influence of its random thermal motions, to return to the most probable configuration, which is characterized by a certain degree of folding and curling. This represents a tendency of the long chain to contract if it is once extended.

The *rate* at which it contracts, however, will not depend upon how much more probable the final (contracted) configuration is than the original (extended), but upon how fast the chainlike molecule can pass from the extended into the curled-up state, which in turn will depend upon

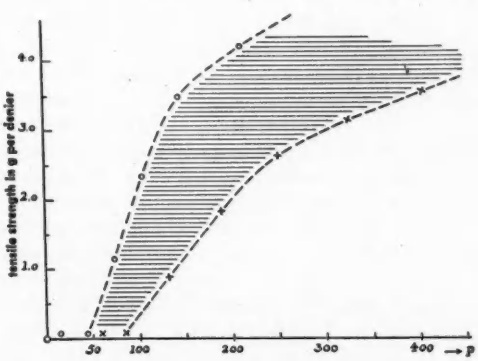


FIG. 2. Relation between tensile strength of polymers and degree of polymerization.

[2] E. Ott, *Cellulose and its derivatives* (Interscience Publishing Co., 1943), p. 930; or R. Houwink, *Elasticity, plasticity and structure of matter* (Oxford, 1939).

TABLE II. Tensile strengths of a few representative fibers of different chemical composition.

Fiber	Chemical composition	Tensile strength (lb/in.²)	Tensile strength (gm/grex)*
Flax	Cellulose	100,000–150,000	6–8
Cotton	Cellulose	35,000– 70,000	2.0–4.5
Textile rayon	Cellulose	25,000– 45,000	1.5–2.5
Tirecord rayon	Cellulose	60,000– 80,000	3.5–4.5
Native silk	Protein	50,000– 80,000	3.5–5.5
Native wool	Protein	20,000	*ca.* 1.5
Acetate rayon, regular	Cellulose acetate	20,000– 30,000	1.5–2.0
Acetate rayon, special	Cellulose acetate	up to 90,000	up to 6.0
Nylon, regular	Polyamide	75,000	5.5
Nylon, special	Polyamide	95,000	7.0

* A *grex* is a metric measure for the cross-sectional area of a fiber, introduced by A. E. Scroggie; it is the weight in grams of 10,000 m of the filament.

how quickly the segments of the molecule can swing from one geometric position to another that is separated from the first by a certain energy barrier. Thus, the *rate of contraction* of a polymer is a matter of the rate of *chain segment diffusion* in the face of energy barriers that separate the possible positions of the individual chain segments from one another. In an isolated long chain molecule these barriers depend upon how much the free rotation about the consecutive bonds in the chain is hindered, either by the intrinsic rigidity of the bond or by the mutual interaction of substituents. It must be added that in an actual piece of rubber there are no isolated chain molecules, but a condensed multitude of them in a state of irregular entanglement, attracting each other with intermolecular (van der Waals) forces. This *mutual* interaction also contributes to the hindrance of the free mobility of the individual molecules and their segments and hence affects the rate of contraction of the material.

There is another aspect of the hindered internal flexibility of the molecules in a polymer. The reason for this hindrance is the existence of energy barriers which have to be overcome as the segments move into less and less geometrically biased positions. Each elementary step of recovery, therefore, requires a certain *activation energy*, which is responsible for the influence of temperature on the rate of segment diffusion and hence on the rate of recovery. If the energy barriers over which the segments have to go are high, it will, at low temperature, become more and more difficult for an individual segment to acquire this energy and the rate of recovery will be considerably reduced. This means that the material will be slow in adapting itself (by segment movement) to the external stress, and hence it will develop *brittleness*.

Thus we may conclude that the intrinsic flexibility of the individual chains is of great importance for the mechanical characteristics of elastomers and affects mainly the rate of recovery from the extended state and its temperature dependence, particularly the point at which the material begins to exhibit brittleness.

It may be added here that the chemical nature of a given polymer is of great importance for its chemical reactivity, swelling, and solubility. Thus polyhydrocarbons, such as natural rubber, polystyrene, polyisobutylene, and so forth, are very resistant to acids and alkalis, and do not absorb water, but eventually swell and dissolve in hydrocarbons and other typically organic solvents. Polymers containing many hydroxyl groups, such as cellulose or polyvinyl alcohol, are in turn very resistant to the action of organic solvents but absorb moisture and eventually even dissolve in water or in weak alkali. It seems, however, that the *mechanical* properties of polymers depend not so much upon whether the monomer is a hydrocarbon, an ester, a sugar, or an alcohol, but rather upon the chemical characteristics as embodied in the molecular chain structure and expressed by average *DP*, *DP* distribution and internal flexibility of the chain molecules. In fact, Table II shows that fairly strong fibers can be made from materials that are very different chemically.

After having now briefly discussed how the structural details of the individual chains affect the mechanical properties of a polymer, let us pass to the importance of the mutual arrangements of the chain molecules in a sample.

Arrangement of the Chain Molecules and Mechanical Properties of Polymers

It may be worth while to consider very briefly the behavior of an ordinary organic substance. At the freezing temperature a liquid of low molecular weight, such as benzene, toluene or isoprene, undergoes a very distinct change in mechanical properties within a narrow temperature range. Below the freezing temperature ordinary substances are crystalline solids; they respond to shearing stress by undergoing small (1 percent or less), predominantly *reversible* deformations and exhibit the long range order (100A or more) of a three-dimensional crystal lattice. Each molecule is held by attractive and repulsive forces of all surrounding particles in a relatively

well-defined equilibrium position, about which it performs rapid, quasi-harmonic vibrations. Transition of an individual molecule from one equilibrium position to another (self-diffusion) is extremely infrequent, the average amplitude of the vibrations amounting only to about 5 percent of the distance between adjacent equilibrium positions. Such systems appear to us as rigid, hard substances, having a definite shape and all other characteristics of a solid body.

As the temperature is increased, the vibrations of the individual particles become more and more vigorous until, at the melting temperature, the forces between the constituents of the lattice can no longer maintain the long range order of the crystal, and the whole structure breaks down. The result is a liquid, characterized by the absence of any far-reaching geometric order. The nearest neighbors of any individual molecule, it is true, are arranged about the latter in much the same manner as in the crystalline state, but molecules a few Angstroms farther away are distributed practically at random. Each single particle, again, carries out rapid quasi-elastic vibrations about an equilibrium position; but this equilibrium position itself (being a position of minimum potential energy) is not fixed in its location and the molecule undergoes, in addition to vibrations, an irregular *translational Brownian movement*, which makes it change its place rather frequently—self-diffusion is rapid. The lack of long range order prevents a liquid from sustaining shear or stress and from assuming a definite shape; it flows. Eyring, Lennard-Jones and their co-workers have succeeded in giving a very satisfactory explanation of many important properties of liquids on the basis of this picture.[3]

A liquid can be undercooled below its equilibrium melting temperature, thus retaining the characteristics of its disordered (amorphous) geometric structure. The coefficient of viscosity η increases with decreasing temperature, roughly following an exponential relationship,

$$\eta = Ae^{BT},$$

where A and B are constants and T is the temperature, and at a sufficiently low temperature the material becomes hard and brittle; it is then called a *glass*. The transition from rigid glass to viscous liquid frequently occurs in a relatively narrow temperature range (the softening point) with no sudden change in structure and with no discontinuity in the primary thermodynamic variables, such as heat content, free energy and specific volume. Thus in the case of ordinary (low molecular weight) organic substances, the relationship between the crystalline, glassy and liquid states can be represented by the scheme of Table III.

TABLE III. Comparison of the various states in which polymers can exist.

State	Extent of geometric order	Viscosity
Crystalline	Long range (more than 1000A)	High
Glassy	Short range (only a few A)	High
Liquid	Short range (only a few A)	Local viscosity low
Rubber	Short range geometric order plus long range entanglement	High for a macroscopic piece

High polymers consisting of long chain molecules exhibit a more complicated behavior. The strong chemical bonds *inside* the individual chains are seldom, if ever, severed during the normal mechanical deformation of the polymer, such as stretching of rubber or spinning of Nylon, or during the various thermal treatments, such as annealing or molding. The changes in shape which take place during these processes occur at the expense of opened (and reclosed) bonds *between* the individual chains, which are not due to chemical forces, but to the various types of intermolecular interaction, such as hydrogen bridges and van der Waals forces.

Let us, from this point of view, compare the crystal lattice of isoprene with that of stretched (or frozen) rubber and consider what happens if the temperature of both systems is increased. In frozen isoprene, each individual molecule (C_5H_{10}) is located in a definite equilibrium position, about which it undergoes irregular, quasi-harmonic vibrations. The distance between any one molecule and its next neighbor is about 4 to 5A, while the interatomic distances within the molecule are only 1.0 to 1.5A. This elucidates the fact that the C_5H_{10} molecule is held together by strong chemical bonds, having dissociation energies of 70 kcal/mole or more, so that it forms a well-defined unit in the lattice, while the forces between the molecules are of the much weaker, van der Waals type (about 5 to 8 kcal/mole). Nevertheless, below the melting temperature, the bonds between molecules suffice to maintain the long range order of a crystal lattice, and frozen isoprene is a hard, rigid substance. Above the melting temperature, however, *all* long range

[3] See, for example, S. Glasstone, *Textbook of physical chemistry*, or *Physical chemistry of high polymers* (Interscience Publishing Co., 1941), p. 177.

connections between the molecules disappear and the result is a liquid with rapid self-diffusion of the individual molecule and small coefficient of viscosity (about 10^{-2} poise).

In frozen rubber, each individual isoprene residue (C_5H_8), as well as each C and H atom within it, vibrates about a definite equilibrium position; but a thorough investigation of the lattice shows that each isoprene residue is particularly close to two other residues, indicating that long linear chains of residues exist, within which *all* next-neighbor distances correspond to strong chemical bonds. The main valence chains of isoprene extend parallel to the direction of the stress, have a length of several thousand monomers, and represent the backbone of the whole structure. In all directions perpendicular to the axes of the chains the distances between adjacent molecules are determined by normal van der Waals forces. This lattice, therefore, is highly anisotropic, with strong forces along the parallelized chains, and weak forces perpendicular to them. As the temperature is increased, the weak intermolecular bonds gradually are severed and thus the lateral arrangement of the chains is disturbed and eventually destroyed. The chains start rotating about their axes, changing their mutual distances and eventually curling and coiling up in an irregular way. However, while the weak bonds *between* the individual chain molecules are opened, the strong (chemical) linkages *within* them remain unaffected. The molecules are not degraded at temperatures at which the lattice structure has already broken down. This has the important consequence that although the *long range geometric order* of the lattice disappears upon raising the temperature to the softening point, a *long range entanglement* due to the unaltered existence of the long chain molecules is maintained. This long range entanglement is responsible for the fact that, at the softening point of a polymer, we get not a liquid, but a *rubbery solid*.

An individual isoprene residue in (amorphous) unstretched rubber vibrates with about the same intensity as does an isoprene molecule at the same temperature in liquid isoprene, and it will also carry out about the same short range Brownian movement; but, owing to its position in the chain molecule, it will not be able to diffuse farther away from its original positions without affecting other parts of the chain to which it belongs. This geometric restriction of the various segments of the flexible linear macromolecules due to the strong bonds between them produces a long range entanglement, which enables the material to sustain moderate stresses or shears, hence to maintain a certain definite volume and shape and resist elastically any attempt at deformation. In this sense rubbers have a state between the solid and liquid, just as glasses do, and one may say that glasses are liquids with high viscosity, whereas rubbers are liquids with long range entanglement (see Table III). The self-diffusion of chain segments is not essentially affected by the long range entanglement and, therefore, is rapid, whereas any displacement of the large linear molecules *as a whole* is impeded by their mutual interaction over long ranges and therefore is very slow. The irregular motion of portions of the chains under the influence of the temperature has been termed, by W. Kuhn, *internal*, or *micro-*, Brownian movement; the displacement of the macromolecules as a whole is referred to as *external*, or *macro-*, Brownian movement.

Using these terms, we can say briefly (and with a certain degree of oversimplification) that rubbers are materials that have rapid internal but slow external Brownian movement. It is this *combination* that characterizes the rubbery state.

Significance of the Two Different Brownian Movements

Let us first consider for which essential feature of rubberiness the two types of Brownian or molecular motion are responsible. If we start stretching a rubber, we expect it to begin deforming at comparatively low stresses; it is supposed to be a soft, extensible material, having a small initial Young's modulus. For typical elastics (soft rubbers or gum stocks), Young's modulus is between 10^6 and 10^7 dyne/cm^2 (15 to 150 lb/in.^{2}). If a material is to extend appreciably under the influence of such small stresses, it needs a considerable degree of internal mobility, much like a normal liquid. In fact, rubbers have many properties in common with ordinary liquids. They possess a compressibility very similar to that of liquids. Poisson's ratio for all "soft" elastics is in the neighborhood of 0.50; the thermal expansivity of rubber is of the same order of magnitude as that of ordinary liquids, and (perhaps most surprising) the solubility of gases (hydrogen, oxygen, etc.) and solids (sulfur, selenium, etc.) in rubbers resembles rather closely the solubility of the same materials in ordinary liquids. The

"local" fluidity of elastics produced by the fast internal Brownian motion is also responsible for this *rapid* contraction. In a stretched rubber certain segments of the individual chains assume configurations the free energy of which is larger than in the unstretched condition. Therefore, as soon as the external force ceases to act, these portions will start to diffuse back into their unstrained (equilibrium) positions, which correspond to a minimum free energy and represent the relaxed state of the sample. How quickly this contraction takes place depends upon the *rate of segment diffusion*, which determines the "local" fluidity of the material.

Certain materials "snap" back into their original shape, because segment motion is fast; others "creep" back, because the local mutual interaction of the chain segments is strong and prevents them from assuming their unstrained positions within a short time. Natural rubber, neoprene, Butyl-rubber and the Buna types are examples of the first case; polystyrene above 80°C, Vinylites and moist polyvinyl alcohol, of the other. In order to get a good, snappy elastic, one has to keep the internal Brownian motion as rapid as possible, which means that one has to accentuate the *local liquid* character of the system.

On the other hand, if we provide in an elastomer for nothing but this high local fluidity and keep a macroscopic sample of the material under stress for a certain length of time, the material will *flow*. It will not sustain the imposed stress or shear, but, with the very aid of the rapid internal Brownian motion, will relax into the extended state instead of into the contracted. Hence it will behave like a viscous liquid or plastic rather than like a rubber. To prevent this permanent loss of shape, one has to provide for strong long range entanglement, which makes the external Brownian movement (slipping of whole chains along one another) so slow that it cannot produce any appreciable *permanent set* within the time the sample is in its extended state. In general, the mutual attraction of the chain by van der Waals forces alone does not provide for a sufficiently stable long range entanglement to conform with practical requirements. Therefore, one usually produces a system of irregularly distributed, widely spaced *fix points*. They provide a stable, but highly deformable network throughout the sample, which can be stretched several hundred percent and will return to its original shape, because its individual knots are connected with one another by flexible chain molecules. Such fix points can be made in different ways. One way is to produce strictly localized, strong chemical bonds between the individual chains, such as sulfur, oxygen, or methylene bridges, as is presumably done during the various processes of curing and vulcanizing. Another way is to have groups of particularly strong molecular interaction (hydrogen bonds, strong dipoles, highly polarizable groups) or of great bulkiness (phenyl, benzyl or naphthyl groups) randomly distributed along the chains, thus producing an irregular network of areas with high molecular adhesion. Still another method is to distribute very small particles with high adsorption power (active fillers) in the polymer, which provide for fix points by adsorbing parts of the mobile chains irreversibly on their active surfaces.

In all these cases, the localized and infrequent strong links between the individual flexible chains suppress the external Brownian movement sufficiently to prevent permanent flow of the sample as a whole, while they leave the internal Brownian movement sufficiently unaffected to allow rapid extension and contraction to be possible.

This discussion shows that a polymer can exist not in two but in three condensed states: the solid, the rubbery and the liquid. In the solid state (crystal or glass) both Brownian movements are frozen in; in the rubbery state the internal Brownian motion is activated while the external Brownian movement is still frozen in; and, finally, in the liquid state of a polymer (in the melt or in a highly swollen sample) both Brownian movements are activated. Under the influence of an external force the solid state has only short range, high modulus elasticity, the rubbery state has long range, low modulus elasticity and the liquid state provides for flow. An ordinary organic substance has only two condensed states, the solid and the liquid; there exists only one type of Brownian motion, which is activated in the latter and frozen in the former state. These two phases, solid and liquid, are in equilibrium with each other at the freezing (or melting) temperature. In the case of polymers there are two characteristic temperatures (or temperature ranges): the *brittle temperature*, which separates the solid from the rubbery state, and the *flow temperature*, which separates the latter from the liquid. Table IV attempts to give a schematic picture of this situation.

TABLE IV. Transition points for ordinary substances and high polymers.

(a) Behavior of ordinary substances:

	Melting point	
Long range molecular order		Short range molecular order
No Brownian motion		Rapid Brownian motion
Sustains external forces; exhibits short range, high modulus elasticity		Does not sustain external forces; flows and has low viscosity

(b) Behavior of polymers:

	Brittle point	*Flow point*
Solid state	*Rubbery state*	*Liquid state*
Long range molecular order	Short range molecular order, but long range entanglement	Short range molecular order
Both Brownian movements are frozen in	Internal Brownian motion rapid; external Brownian motion still frozen in	Both Brownian motions are activated
Sustains external forces; exhibits short range, high modulus elasticity	Sustains external forces; exhibits long range, low modulus elasticity	Does not sustain external forces; flows and has medium viscosity

Significance for the Mechanical Properties of Polymers

Considering the facts as discussed in the preceding section, it becomes clear that, in order to produce a polymer for practical purposes, one has to synthesize chain molecules with a *DP* around 1000, having a certain degree of internal flexibility. This material has then to be brought into the proper physical state or mixture of states. And here we realize that we have at our disposal *three* different phases: the solid, the rubbery and the liquid. The task of bringing the polymer into the best physical state is similar to that of a metallurgist, who attempts to find the best properties of his alloy in the phase diagram of his system, with the difference that this organic chemical "metallurgy" puts one more state at his disposal, namely, the rubbery. This is because of the presence of the long chain molecules.

It is now obvious that, if the desire is to produce a strong and resilient fiber, one will have to accentuate the solid state in order to get stiffness and tenacity and will have to provide for only as much rubberiness as will give a sufficient resiliency. Any liquid constituent in such a fiber would be unwelcome because of the permanent set to which it leads under prolonged external stress. If one aims at the production of a typical rubber, it will be necessary to have the bulk of the material in the rubbery state, with perhaps some crystalline domains in order to provide for a sufficiently strong fix-point system. Finally, in producing a plastic, one will have to provide for an appropriate mixture of solid and liquid material with as little rubberiness as possible in order to guarantee smooth extruding, casting or molding without recovery after the desired shape has been obtained.

In this sense it can be said that fibers, plastics and rubbers are not intrinsically different materials: they are merely different combinations of the three fundamental states in which organic high polymers can appear; and there exists, obviously, a continuous range of systems, starting with extreme fiber properties and ending with extreme rubber qualities, depending essentially upon the degree to which the solid, rubbery and liquid state are represented in the sample.

Hence, there are two independent ways to arrive at new polymers with novel and interesting mechanical properties: (i) to prepare chemically new monomers and produce long chain molecules of them with a high *DP* and a favorable molecular weight distribution, and (ii) to bring these substances into the proper physical state by adjusting the proportions of the solid, rubbery and liquid constituents such that they incorporate to the best degree the desired properties. Along both lines a large number of research chemists and physicists are active, and there can be little doubt that their work will lead to an ever-increasing number of new materials with valuable and surprising properties.

OUR present era is characterized by something new in the life of man, and that is the impact of science and applied science or technology on our lives. However, our ultimate goal is not science, just for science's sake; our goal is a higher degree of culture and civilization. We should realize that science is not the measure of civilization—science and technology are merely tools, not ends in themselves.—GASTON F. DU BOIS.

Accuracy of Constants in Exponential Decay as Obtained from Finite Samples—A Review

PAUL L. COPELAND
Illinois Institute of Technology, Chicago, Illinois

NUMEROUS physical problems involve a law of transformation similar to that of radioactive decay, in which out of an initial quantity q_0 the amount q surviving unchanged after time t is given by the relationship

$$q = q_0 \exp(-kt), \tag{1}$$

or

$$\ln(q/q_0) = -kt,$$

where k is a constant which may be determined by experiment. In some cases, such as radioactive decay or the discharge of a condenser through a resistor, the accuracy with which the constant can be determined depends primarily on the accuracy with which suitable instruments are read. The discreteness of the quantities involved —electric charge in the examples just cited—does not play a significant role. On the other hand, if one studies the survival of certain types of equipment by recording the lives of units chosen at random, it may be burdensome or prohibitively expensive to increase the number of units to such an extent that statistical fluctuations cease to limit the accuracy. A somewhat similar situation is encountered in the study of gas discharges when the duration of a discharge under specified conditions is studied by means of repeated trials. In some cases it may be quite impractical to increase the number of observations until statistical fluctuations are negligible, and a knowledge of the accuracy to be expected from the size of the sample used may be important for an interpretation of the result.

The Exponential Decay Function

For the computation of the decay constant k or of its reciprocal, the average life, the relationship to be used is

$$k = \ln(q_0/q)/t. \tag{1'}$$

For present purposes it may be assumed that the accuracy of k will not be limited by errors in t. There may be uncertainties in both q and q_0, but these may be combined by the usual rules to give the uncertainty of the ratio; this is expressed in the usual way by writing $(q_0/q)\pm\epsilon$, where ϵ stands for the probable error. Thus

$$k = \ln[(q_0/q)\pm\epsilon]/t. \tag{2}$$

Expanding the numerator in a power series, we obtain

$$\ln(q_0/q\pm\epsilon) = \ln q_0/q + 2\{\pm\epsilon/\langle(2q_0/q)\pm\epsilon\rangle + \tfrac{1}{3}[\pm\epsilon/\langle(2q_0/q)\pm\epsilon\rangle]^3 + \cdots\}. \tag{3}$$

If ϵ is small compared with q_0/q, only the first term in the series needs to be retained, and even in it ϵ may be neglected in comparison with q_0/q. Hence a good approximation is

$$\ln[(q_0/q)\pm\epsilon] = \ln(q_0/q)\pm\epsilon/(q_0/q). \tag{4}$$

This means that the absolute error in the logarithm of a quantity is equal to the relative error in the quantity itself. The relative error r in the logarithm is given by

$$r = \pm\epsilon/(q_0/q)\ln(q_0/q). \tag{5}$$

Thus the relative error in the quantity $\ln[(q_0/q)\pm\epsilon]$ is the relative error in the argument itself divided by $\ln(q_0/q)$. Even though the argument has a small relative error, the error in k will be large when q is almost equal to q_0 so that $\ln(q_0/q)$ is small. The relative error in k will be the same as that in the ratio when q_0/q is equal to the base of natural logarithms. On the other hand, the relative error in k will be only one tenth the relative error in q_0/q when the logarithm is equal to ten. This condition corresponds to a value of 20,026 for the ratio, and in some laboratory work this magnitude can be attained. This situation justifies carrying one more figure in k than in the ratio q_0/q.

When, for the purpose of determining k, it is observed that in a small, exactly known time interval Δt the quantity q decreases from q_0 by a small increment Δq in which the relative error is $\epsilon/\Delta q$, then the relative error in k is the same as the relative error in the increment. In this case

$$k = [\ln q_0/(q_0-\Delta q\pm\epsilon)]/\Delta t.$$

Expanding the argument of the logarithm in a

power series and retaining only terms of the first order of the quantities ϵ and Δq, we obtain

$$k=\ln\left(1+\frac{\Delta q}{q_0}\mp\frac{\epsilon}{q_0}+\cdots\right)\Big/\Delta t.$$

Expanding the logarithm in a power series and retaining only first-order terms, we obtain

$$k\cong\Delta q/q_0\Delta t\mp\epsilon/q_0\Delta t. \tag{6}$$

Division of the probable error in k, which from Eq. (6) is seen to be $\epsilon/q_0\Delta t$, by the magnitude of k, which is $\Delta q/q_0\Delta t$, gives for the relative error in k,

$$r=\epsilon/\Delta q,$$

which is just the relative error in the increment Δq.

Statistical Fluctuations in Small Samples

Fluctuations in small samples arising from statistical considerations are well understood.[1] If N_0 units are distributed according to chance in such a way that the average number falling in the ith class is $\langle N_i\rangle_{Av}$, the mean-square departure of the number N_i observed in individual trials from $\langle N_i\rangle_{Av}$, which is denoted by $\langle\Delta^2\rangle_{Av}$ and called the *variance*, the *dispersion* or the *statistical fluctuation*, is given by

$$\langle\Delta^2\rangle_{Av}\equiv\langle(N_i-\langle N_i\rangle_{Av})^2\rangle_{Av}=\langle N_i\rangle_{Av}(1-\langle N_i\rangle_{Av}/N_0), \tag{7}$$

and the *standard deviation* σ is the root-mean-square departure from the average,

$$\sigma=(\langle\Delta^2\rangle_{Av})^{\frac{1}{2}}. \tag{8}$$

The probabilities of various distributions are readily found. Let P represent the probability of finding any one of the units in the ith class, and let Q represent the probability of finding it in all the rest of the classes combined, so that $\langle N_i\rangle_{Av}=PN_0$ and $P+Q=1$. Then the probability p_u of a distribution differing from the average by u individuals in the ith class is given by the relationship

$$p_u=\frac{N_0!P^{N_0P+u}Q^{N_0Q-u}}{(N_0P+u)!(N_0Q-u)!}. \tag{9}$$

If N_0 is very large and $P=Q$, p_u approximates the familiar Gauss error curve, the form of which is given by

$$p_u=(1/\sigma\pi^{\frac{1}{2}})\exp(-u^2/2\sigma^2). \tag{10}$$

If P is not nearly equal to Q, the distribution is unsymmetrical, and if, in addition, $\langle N_i\rangle_{Av}$ is not very large, the asymmetry is pronounced. Whenever $\langle N_i\rangle_{Av}$ is small, the discrete steps in the distribution function are quite marked.

The properties of the Gauss error curve are well known. A maximum appears at $u=0$, and the ordinates representing probability decrease, at first slowly and then more rapidly, as u^2 increases. The curve is symmetrical about the line $u=0$. The integral representing the area under the curve is usually called the *error function*, and it has been evaluated for various limits. In particular, these calculations have shown that the *probable error*, which is an error of such magnitude that larger and smaller errors are equally likely, is 0.6745 times the standard deviation:

$$\epsilon=0.6745\sigma=0.6745[\langle N_i\rangle_{Av}(1-\langle N_i\rangle_{Av}/N_0)]^{\frac{1}{2}}. \tag{11}$$

This result would be expected to apply whenever both N_0P and N_0Q are large. For the special case $P=Q$,

$$\epsilon=0.6745[\tfrac{1}{2}N_0(1-\tfrac{1}{2})]^{\frac{1}{2}}=0.338N_0^{\frac{1}{2}}. \tag{12}$$

The coefficient 0.338 is surprisingly accurate even for small samples.[2]

TABLE I. Values of p_u as a function of u when N_0 is 100.

u \ $\langle N_i\rangle_{Av}=N_0P$	1	2	3	5	10
−9					0.0003
−8					.0016
−7					.0058
−6					.0158
−5				0.0059	.0338
−4				.0312	.0594
−3			0.0475	.0812	.0887
−2		0.1326	.1471	.1396	.1146
−1	0.3660	.2705	.2252	.1781	.1302
0	.3697	.2732	.2275	.1800	.1318
1	.1848	.1822	.1706	.1500	.1198
2	.0609	.0902	.1013	.1059	.0988
3	.0149	.0353	.0495	.0649	.0744
4	.0029	.0114	.0206	.0349	.0516
5	.0005	.0032	.0074	.0167	.0331
6		.0008	.0024	.0073	.0196
7		.0002	.0007	.0028	.0108
8			.0001	.0010	.0056
9				.0003	.0027
10					.0012

[1] R. B. Lindsay, *An introduction to physical statistics* (Wiley, 1941), p. 15.

[2] For example, if $P=Q=\frac{1}{2}$ and N_0 is 2, then by the use of Eq. (9) we obtain $p_{-1}=\frac{1}{4}$, $p_0=\frac{1}{2}$ and $p_1=\frac{1}{4}$. If division points are placed halfway between the integral values, from the actual distribution of probability, we find that $\epsilon=\frac{1}{2}$. According to Eq. (12), $\epsilon=0.477$.

Sometimes the curve representing probability differs from the normal. In the first place, the distribution may be unsymmetrical, and the asymmetry is measured by the *skewness* α_3, which is defined as the third moment of the distribution about its mean divided by the cube of the standard deviation. Of course, the skewness of the normal curve is zero. In the second place, there is the *flatness*, or *kurtosis*, of the distribution. This is denoted by α_4 and is defined as the fourth moment of the distribution about its mean divided by the fourth power of the standard deviation. The kurtosis of the normal curve is 3.

The velocity distribution for the molecules of an ordinary gas provides the most familiar illustrations of the foregoing principles. If the number of molecules having x-components of velocity within a unit range about a value v_x is plotted as a function of v_x, the graph is a normal curve symmetrical about $v_x=0$. The skewness is obviously zero, and the kurtosis of 3 can be verified readily by calculation. On the other hand, the graph of the number of molecules having speeds in unit range about v as a function of the speed v is the unsymmetrical bell-shaped curve which represents the equation

$$p_v=(4v^2/\alpha^2\pi^{\frac{1}{2}})\exp(-v^2/\alpha^2), \tag{13}$$

where α is the most probable speed. The average speed is $2\alpha/\pi^{\frac{1}{2}}$. The root-mean-square speed is $\alpha\frac{3}{2}^{\frac{1}{2}}$. The variance is $\alpha^2[\frac{3}{2}-4/\pi]$, or approximately $0.228\alpha^2$, and the skewness of the distribution in speed is approximately $+\frac{1}{3}$. Curves representing these distributions are found in some general physics textbooks and in books on kinetic theory.

Probabilities associated with small classifications in a sample of 100 units, as calculated from Eq. (9), are shown in Table I. The properties of the probability distribution represented by Eq. (9) are summarized in the following formulas.[3]

$$\langle N_i\rangle_{Av}=N_0P, \tag{14}$$

$$\sigma=(PQN_0)^{\frac{1}{2}}, \tag{15}$$

$$|\alpha_3|=(P-Q)^2/PQN_0, \tag{16}$$

$$\alpha_4=3+(1-6PQ)/PQN_0. \tag{17}$$

Perhaps the most striking feature of the results shown in Table I is the marked difference in probability for positive and negative values of u. The classification is almost as likely to contain one less unit than the average as it is to contain

[3] Shewhart, *Economic control of quality* (Van Nostrand, 1931), p. 153.

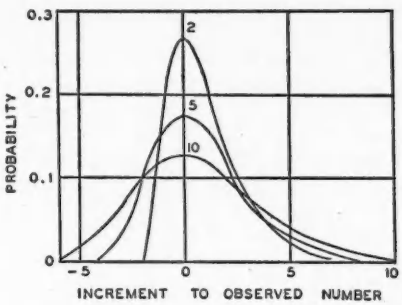


FIG. 1. Probability of the classification size differing from a single observation. The sample size is 100. The number of units observed in the classification is shown by the numeral at the maximum of the curve.

the average number itself. On the other hand, the classification is relatively unlikely to contain one unit more than the average number. To compensate for this, the probability of large positive departures remains finite, whereas the negative deviations giving less than zero units in the classification are associated with zero probability. These differences for positive and negative values of u, which are especially marked when $\langle N_i\rangle_{Av}$ is not large, mean that the similarity to a normal curve is not close; but Eqs. (16) and (17) show that, as N_0 becomes large, properties associated with the normal curve are approached.

It may be impossible in practice to make a classification in such a way that the exact average is known. Ordinarily one selects the rule for making the classification and then observes the number of units falling within the classification. One does not ordinarily know the number of units that should have been observed and is interested in knowing the likelihood of this observation being correct and also the probabilities corresponding to definite errors. For the purpose of obtaining such information, Eq. (9) suffices. In these calculations the numbers given by the observed division of the units are maintained constant, and the quantities P and Q are varied in accordance with the condition $P+Q=1$. Results thus obtained are shown in Fig. 1, where the ordinates represent probabilities p_u and the abscissas represent the differences u between the observed number of units and the average $\langle N_i\rangle_{Av}$. In general, these results closely resemble normal Gauss error curves. The observed number of units is most likely to be close to the average expected from a large number of similar trials. The curves show that large errors in estimating

from a single observation the size of the classification are relatively unlikely. When the number of units observed is as small as two, the asymmetry is marked, but when it is as large as five, the curve is nearly symmetrical. Equation (11) gives the probable error in the observation with high accuracy when five or more units are found in the classification. For example, when five units are observed, putting five for $\langle N_i \rangle_{Av}$ in Eq. (11) gives 1.507 units for the probable error, whereas the value based on the form of the curve shown in Fig. 1 as determined by numerical integration is 1.473 units.

Distribution of Errors in the Average Life of Finite Samples

In a large sample of q_0 units, the number of units likely to transform per unit time at time t may be found by taking the negative of the derivative of Eq. (1):

$$-\mathrm{d}q/\mathrm{d}t = kq_0 \exp(-kt). \tag{18}$$

If q_0 is made equal to unity, $-(\mathrm{d}q/\mathrm{d}t)$ represents the probability that a unit will transform in a time interval of unit length at the time t. The expected value for the life of a single unit is defined by

$$\mathrm{E}(t) = \int_0^\infty t[k \exp(-kt)dt] = 1/k. \tag{19}$$

The integral in this case is also recognized as the definition of average life. Thus

$$\mathrm{E}(t) = 1/k = \langle \bar{t} \rangle_{Av}. \tag{19'}$$

The symbol $\langle \bar{t} \rangle_{Av}$, suggesting the average of averages, is used to stand for the average of the individuals in the "universe," so that $\bar{t}$ may be used to stand for the average of a single sample. The dispersion in the lives of the individual units is found by a similar calculation based on the squares of the departures from the mean life. This is the second moment of the distribution about the line representing the mean life:

$$\mathrm{E}(\Delta^2) = \int_0^\infty (t - \langle \bar{t} \rangle_{Av})^2[k\mathrm{d}t \exp(-kt)] = \langle \bar{t} \rangle_{Av}^2. \tag{20}$$

The square root of the dispersion is the standard deviation of the individual units from the average life:

$$\sigma = \langle \bar{t} \rangle_{Av}. \tag{21}$$

The third and fourth moments, denoted by μ_3 and μ_4, respectively, are of interest:

$$\mu_3 = \int_0^\infty (t - \langle \bar{t} \rangle_{Av})^3[k\mathrm{d}t \exp(-kt)] = 2\langle \bar{t} \rangle_{Av}^3, \tag{22}$$

$$\mu_4 = \int_0^\infty (t - \langle \bar{t} \rangle_{Av})^4[k\mathrm{d}t \exp(-kt)] = 9\langle \bar{t} \rangle_{Av}^4. \tag{23}$$

The ratio μ_3/σ^3, which gives the skewness, is found to be 2. The ratio μ_4/σ^4, which gives the kurtosis of the distribution, is found to be 9. The deviations of the exponential curve from the normal error curve are very large. The exponential curve is very skew, and, because the distribution ends abruptly at $t=0$, an extremely large value of kurtosis is obtained.

The probability of various averages arising from two observations is easily calculated. The probability $\mathrm{F}_1(t)\mathrm{d}t$ that an observation of a single life lies between t and $t+\mathrm{d}t$ is given by

$$\mathrm{F}_1(t)\mathrm{d}t = k\mathrm{d}t \exp(-kt). \tag{18'}$$

To obtain the value t' as the average of two readings it is necessary that if the one reading be $t'+x$ the other should be $t'-x$. The probabilities of these separate events are

$$\mathrm{F}_1(t'+x)\mathrm{d}t = k\mathrm{d}t \exp[-k(t'+x)]$$

and

$$\mathrm{F}_1(t'-x)\mathrm{d}t = k\mathrm{d}t \exp[-k(t'-x)],$$

where $-t' \leqslant x \leqslant t'$. The probability of the two occurring together is the product of the two individual probabilities, or

$$k^2(\mathrm{d}t)^2 \exp(-2kt'),$$

which fortunately is independent of x. To determine the probability $\mathrm{F}_2(\bar{t}')\mathrm{d}\bar{t}$ that the average of two lives will lie in a range $\mathrm{d}\bar{t}$ at $\bar{t}'$, we notice that the first life may lie anywhere within the range $-\bar{t}' \leqslant x \leqslant \bar{t}'$. Once a definite life $\bar{t}'-x$ in this range is observed, an average in the desired range may be obtained by taking the other in the range $\bar{t}'+x-\mathrm{d}\bar{t} \leqslant t \leqslant \bar{t}'+x+\mathrm{d}\bar{t}$. If we take account of the allowed ranges of $2\bar{t}'$ and $2\mathrm{d}\bar{t}$ which give rise to the probability $\mathrm{F}_2(\bar{t}')\mathrm{d}\bar{t}$, we have

$$\mathrm{F}_2(\bar{t})\mathrm{d}\bar{t} = 4k\bar{t}\mathrm{d}\bar{t} \exp(-2k\bar{t}). \tag{24}$$

In this equation the prime has been dropped from the argument, because the equation holds for any positive value of $\bar{t}$. The function $\mathrm{F}_2(\bar{t})$ is

plotted in Fig. 2. Although the exponential $F_1(t)$ does not resemble a Gauss error curve, $F_2(\bar{t})$ is marked by several obvious similarities to a normal distribution.

It is not difficult to determine the probabilities associated with various averages for samples consisting of larger numbers of units. The averaging process involves adding the individual lives, and so does the process of multiplying the exponentials that express the individual probabilities. Thus the exponential factor for a sample containing N units giving the average $\bar{t}$ is always $\exp(-N\bar{t}\lambda)$, regardless of the individual lives from which it arises. It is noticed, however, that for the average life of $N-1$ units there is a finite range of values which may be paired with permissible values for the life of the Nth unit to give an average life for the N units falling in the range $d\bar{t}$ at $\bar{t}$. Taking account of this fact involves replacing $d\bar{t}$ in the expression for $F_{N-1}(\bar{t})$ by a finite range expressed by $c'\bar{t}$, where c' is a constant depending on N. Thus, in general,

$$F_N(\bar{t})d\bar{t} = C\bar{t}^{N-1}d\bar{t}\exp(-N\lambda\bar{t}). \quad (25)$$

The value of the constant C is obtained directly if the details are carried throughout the argument establishing Eq. (25) by mathematical induction. It is much easier to evaluate C by the normalizing condition on probability, namely,

$$\int_0^\infty F_N(\bar{t})d\bar{t} = 1. \quad (26)$$

The general theorems of mathematical statistics[4] suffice to establish the result directly, and it is found to be

$$F_N(\bar{t}) = N^N\lambda^N\bar{t}^{N-1}\exp(-N\lambda\bar{t})/(N-1)!. \quad (27)$$

Regardless of the size of N, this expression satisfies Eq. (26). The expected value of the average life $\bar{t}$ for a sample is given by

$$E(\bar{t}) = \int_0^\infty \bar{t}F_N(\bar{t})d\bar{t} = \langle\bar{t}\rangle_{Av},$$

and the result is independent of sample size.

The values of $F_4(\bar{t})$, as determined from Eq. (27), are plotted in Fig. 2. Although this curve is somewhat unsymmetrical, it suggests the rapidity with which the form of the normal curve is being approached as the size of the sample is

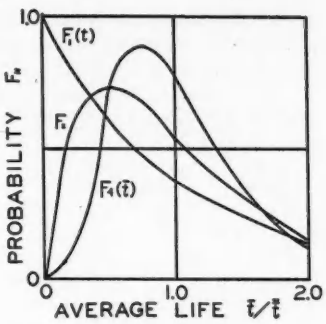


FIG. 2. Probability of various averages for samples from an exponential population.

increased. The value of $\bar{t}$ corresponding to the maximum t_m of the probability curve, as found by differentiating $F_N(\bar{t})$ with respect to the argument and setting the result equal to zero, is given by

$$t_m = \langle\bar{t}\rangle_{Av}(N-1)/N.$$

Thus the most likely average $\bar{t}$ for the sample approaches $\langle\bar{t}\rangle_{Av}$ quite rapidly as the number of individuals in the sample is increased.

By the use of the formula

$$\int_0^\infty x^n \exp(-ax)dx = n!/a^{n+1},$$

the following relationships are easily verified:

$$\mu_{2:\bar{t}} \equiv \int_0^\infty (\bar{t}-\langle\bar{t}\rangle_{Av})^2F_N(\bar{t})d\bar{t} = \langle\bar{t}\rangle_{Av}^2/N, \quad (28)$$

$$\mu_{3:\bar{t}} \equiv \int_0^\infty (\bar{t}-\langle\bar{t}\rangle)^3F_N(\bar{t})d\bar{t} = 2\langle\bar{t}\rangle_{Av}^3/N, \quad (29)$$

$$\mu_{4:\bar{t}} \equiv \int_0^\infty (\bar{t}-\langle\bar{t}\rangle_{Av})^4F_N(\bar{t})dt = (3N+6)\langle\bar{t}\rangle_{Av}^4/N^3. \quad (30)$$

Hence the standard deviation s for the average of a sample is given by

$$s = (\mu_{2:\bar{t}})^{\frac{1}{2}} = \langle\bar{t}\rangle_{Av}/N^{\frac{1}{2}}. \quad (31)$$

The fact that the standard deviation of means for samples of size N is the standard deviation of individuals in the population divided by the square root of the number of individuals in the sample is true for all distributions with finite

[4] A. T. Craig, Am. J. Math. 54, 353 (1932).

second moments,[5] and it is the basis of the familiar rule for determining the probable error of the average of N observations of equal weight.

By the use of moments and coefficients based on the exponential population, the following formulas are readily established:

$$\alpha_{3:i}=\mu_{3:i}/s^3=\alpha_3/N^{\frac{1}{2}}, \tag{32}$$

$$\alpha_{4:i}=\mu_{4:i}/s^4=3+(\alpha_4-3)/N. \tag{33}$$

These also are general results[5] which hold for all distributions with finite moments. They express quantitatively the approach to the normal curve as the sample size increases.

Small Samples

The exponential decay curve arises from a population in which the likelihood of an individual transforming in unit time is constant until the transformation occurs, and is zero thereafter. On the average, fewer individuals transform at the later times because fewer remain to be transformed. It is this fact that accounts for the exponential character of $F_1(t)$. In a large population, 63.2 percent of the individuals have lives shorter than the average life $\langle t\rangle_{Av}$ and 36.8 percent have lives longer than the exact average.

A sample consisting of a single unit is incapable of giving evidence concerning the nature of the distribution. If, however, the exponential nature of the population has been established, either by previous experiments or by theory, a single observation may be analyzed accordingly. A single observation is most likely to be low, but it is not certain to be low. Apparently one can do no better than to assume that the observed life approximates the average life and that the standard deviation of the population also is represented by the magnitude of the observed life. This information is very inexact and unsatisfactory. It is far less satisfactory than a single observation on a normal distribution, where the most likely observation is the average.

A sample consisting of two units is still incapable of giving evidence that the population is exponential. The skewness arising from the skewness of the parent population persists, and the most likely average for the sample is only half that of the parent population. The distribution function for the range W of N individuals is[4]

$$\varphi_6(W)=(N-1)\{\exp W-1\}^{(N-2)}\exp(1-N)W, \tag{34}$$

which for the special case of two individuals, where $s=\frac{1}{2}W$, gives for the distribution of the deviations $\varphi(s)=2\exp(-2s)$. The observed spread of two observations is very likely to fall short of the value $2\sigma=2$ expected for the population. For such small samples the standard deviation of the sample should not be presented as the estimate of the standard deviation of the parent population. The estimate provided by the formula $s=\bar{t}$ is more reliable.

Since the usual evidence for the exponential law is the constancy of the slope of the graph resulting when the logarithm of the number of survivors is plotted as a function of the time, the smallest sample which could be used as evidence for the exponential law is three units. The identification of the law cannot be made with reasonable certainty on the basis of three observations. As the sample size is increased, the certainty with which the sample establishes the nature of the parent population increases rapidly. The probability that the average of the sample will be close to the average of the population also increases, and the standard deviation of the averages decreases. The skewness of the population seriously interferes with the accuracy of estimates based on small samples, but this difficulty disappears for samples of quite reasonable size. The displacement of the maximum of the probability curve from the correct average is $\langle t\rangle_{Av}/N$, while the standard deviation is $\langle t\rangle_{Av}/N^{\frac{1}{2}}$. Thus the uncertainty due to the size of the standard deviation persists after the uncertainty due to the skewness of the population has become negligible.

A sample as large as ten units gives almost conclusive evidence for the exponential law. When the logarithm of the number of survivors is plotted as a function of the time the slope of the line defined appears to be determined with considerable accuracy. The consequence has been that in many cases workers interested primarily in the application of results have estimated the average life from the slope of the semilogarithmic graph without any investigation of the probable error. Doubtless the regularity of the graphs, which indicates an accuracy greater than that required in the studies, has justified this procedure to those who have used it.

Probable Error of the Average

The exponential population differs so much from a normal distribution that in the case of small samples it is well to modify the usual procedures for estimating the average and the probable error. The average life of the parent population is not that most likely for the sample. The usual rule for determining the probable error is not entirely worthless even when an effort is made to apply it to single observations. The average life of the parent population is $\langle t\rangle_{Av}$, and the standard deviation is also $\langle t\rangle_{Av}$. Integration of the exponential function shows that the percentage of the population lying between the values $(1-0.6745)\langle t\rangle_{Av}$ and $(1+0.6745)\langle t\rangle_{Av}$ is 53.5. Furthermore, although the correct average and deviation are not obtainable from a single observation t, the properties of the exponential population may be used to show that the range from $(1-0.6745)t$ to $(1+0.6745)t$ does include the correct average in the case of 50.096 percent of the observations. The difficulty is that a single observation in this case is quite likely to fall far short of the average and hence to give an unre-

[5] Kenny, *Mathematics of statistics* (Van Nostrand, 1939), book II, p. 103.

liable estimate of both the average life and the standard deviation. A single observation on an exponential population is essentially less reliable than a single observation on a normal distribution. A remarkable thing is the degree to which even two observations will reduce the difficulty. The distribution of the averages approaches a normal curve very rapidly.

If a sample contains less than five units, the average life of the sample should not be accepted uncritically as the best estimate of the average life in the parent population. Results of statistics providing the basis for the interpretation of the lives occurring in small samples are: (i) the probable distribution of average lives, Eq. (27); (ii) the probable distribution of ranges, Eq. (34); and (iii) the probable separation of pairs. If the observed lives are considered in detail and an interpretation of the data is selected which eliminates very unlikely events of all three types, the interpretation is apt to be reliable quantitatively. When the sample contains from five to ten units, the average of the sample is apt to be fairly representative, and the usual rules for probable error, Eqs. (8) and (11), are applicable. For samples containing more than ten units, the regular procedures may be applied with confidence. The average of the sample is likely to be close to that for the parent population. The probable relative error of the result (probable error/average life) is given by $0.6745/N^{\frac{1}{2}}$. Although the skewness is still appreciable when $N=10$, the maximum for the distribution of sample averages does fall within the central half of the range about $\langle t\rangle_{Av}$ established by the limits $\pm 0.6745\langle t\rangle_{Av}/N^{\frac{1}{2}}$. The fundamental difficulty is not with the rules; it is the fact that the rules necessitate so many readings for high accuracy in the case of the exponential population. More than 100 observations are required to reduce the probable error to 6 percent, and 1000 are required if the allowable inaccuracy is 2 percent.

Calculations Based on Proportion Surviving to a Fixed Time

A somewhat simplified, and less exact, method of treating the data may be of interest, because it is very directly related to common calculations, and because sometimes the data obtained permit no other analysis. Let n represent the number of individuals surviving after t sec. Then

$$k=[\ln N-\ln\{n\pm 0.6745(n(1-n/N))^{\frac{1}{2}}\}]/t.$$

TABLE II. Probable errors in exponential decay.

Size of Sample, N	Untransformed Residue, n	Probable Relative Error Eq. (36)	Probable Relative Error $0.6745/N^{\frac{1}{2}}$
20	15	0.32	0.151
	10	.24	
	5	.22	
	2	.27	
50	25	.15	.095
	10	.13	
	5	.15	
100	50	.10	.067
	25	.09	
	10	.10	
200	100	.071	.048
	50	.062	
	25	.063	
	10	.075	

Expanding the second logarithm in a power series, and retaining the entire first term, we obtain

$$k=\left[\ln N-\ln n \pm\frac{2\times 0.6745(n(1-n/N))^{\frac{1}{2}}}{2n\pm 0.6745(n(1-n/N))^{\frac{1}{2}}}\right]\Big/t. \quad (35)$$

The relative error in k due to statistical fluctuations is thus

$$r=2\times 0.6745(n(1-n/N))^{\frac{1}{2}}/\{2n\pm 0.6745(n(1-n/N))^{\frac{1}{2}}\}\ln N/n. \quad (36)$$

Values of relative error have been computed for various values of N and n according to Eq. (36), and the results are shown in Table II. Since the use of the negative sign results in the larger error, this sign is chosen for the calculations. The value of $0.6745/N^{\frac{1}{2}}$ is also shown for each set.

In this latter analysis, the sample is divided into two parts by the selection of a single value of t, and all other information potentially available within the sample is ignored. It is as if, instead of recording the times at which individual units transform, one merely recorded whether the units survived or failed to survive until the time t. In this case the probable relative error, as would be expected, depends not merely on N, but also on how the time selected for the division is related to the average life of the units. The calculations summarized in Table II show that

the results are relatively certain when the selected time results in the survival of roughly one fourth of the units in the sample. Since the analysis ignores some of the information available in the sample, it is not surprising that the relative error, even for this optimum division, is larger than $0.6745/N^{\frac{1}{2}}$.

Illustrations and Applications

Statistical fluctuations have been studied extensively in connection with radioactivity. Rutherford and Geiger[6] discussed the probability variations in the distribution of alpha-particles. A more recent investigation by Curtis[7] has shown that statistical theory accounts for the considerable fluctuations in the radioactivity of weak sources. In these studies the sources were considered constant, because the times over which observations were made were short compared with the half lives of the sources. More recently, sources decaying at various rates have been produced under controlled conditions in experiments on nuclear physics. The interpretation of counter experiments with small samples has stimulated the development of the general theory.[8] In a number of papers[8,9] statistical considerations have been applied specifically to the problems arising in counter experiments.

Random phenomena are involved in gaseous discharges, and the operation of very familiar types of equipment is influenced by probability considerations. Slepian and Ludwig[10] studied the backfires in mercury arc rectifiers, and concluded that they were random phenomena of the type considered here. Dow and Powers[11] studied the firing time of the igniter type tube. They observed repeatedly the times by which the initiation of the arc lagged behind the application of a definite potential gradient to the igniter rod. Plotting the percentage of ignitions occurring later than time t as a function of t, they found that their data fell along curves of the form $\exp(-kt)$. Copeland and Sparing[12] studied the survival of a low pressure arc with a mercury-pool cathode in repeated trials under fixed operating conditions and obtained the same law. This enabled them to make quantitative studies of arc stability as a function of operating conditions.

The time lags in spark breakdown have been studied by Laue[13] and Zuber.[14] An overvoltage sufficient to cause breakdown was applied to a gap under similar conditions in a number of tests, and the distribution of times at which breakdown occurred was found to follow the exponential decay law. Tilles[15] used an improved mechanism for timing short intervals, and made a more extensive investigation. He verified the exponential law for the distribution of the time lags and measured the average time lag under enough different conditions to determine functional relationships between it and operating conditions.

The existence of the exponential decay law is suggested by a wide spread of observations in which the longer times occur with decreasing frequencies. A semilogarithmic graph of such data, if reasonably extensive, is almost certain to settle the question of whether observations are due to random survivals. The varied examples cited in this review suggest the wide applicability of the statistical considerations underlying random phenomena. Fortunately the theory is being extended in research which promises to increase the usefulness of this method of analysis.

[6] Rutherford and Geiger, Phil. Mag. **20**, 698 (1910).
[7] L. F. Curtis, Bur. Stand. J. Research **8**, 339 (1932).
[8] Ruark and Devol, Phys. Rev. **49**, 355 (1936).
[9] R. Peierls, Proc. Roy. Soc. **A149**, 467 (1935).
[10] Slepian and Ludwig, Trans. A.I.E.E. **51**, 92 (1932).
[11] Dow and Powers, Trans. A.I.E.E. **54**, 942 (1935).
[12] Copeland and Sparing, Phys. Rev. **59**, 115 (1941).
[13] M. von Laue, Ann. d. Physik **76**, 261 (1925).
[14] K. Zuber, Ann. d. Physik **76**, 231 (1925).
[15] A. Tilles, Phys. Rev. **46**, 1015 (1934).

I DO not think we can maintain our position in the postwar world unless we are an exceptionally well-educated people and unless we can handle easily and with comprehension the problems and inventions of the new scientific age.—WINSTON CHURCHILL.

American Physicists at War*: from the Revolution to the World Wars

I. Bernard Cohen
Harvard University, Cambridge, Massachusetts

The other humane studies I apply myself to are natural philosophy, the new mechanics and husbandry, according to the principles of our new philosophical colledge that value no knowledge but as it hath a tendency to use.—Robert Boyle

EVERY layman who reads or who listens to the radio knows that the sciences are playing an important part in this war. On this subject the attitude of the research physicist is summed up best, perhaps, in President Conant's statement: "Science is enlisted for the duration. Its effectiveness will be announced to the enemy by action rather than by any news of progress that has been made." Few, if any, physicists lead lives unaffected by the war. A great number of us are busy in secret war work, others know the burdens of Army or Navy teaching, or of increased civilian teaching, many serve in the armed forces.

Even during the course of the war, many physicists, like their fellow scientists in other fields, may well wonder what effect the war will have on scientific research and organization when peace comes. This problem was given a new light when, on November 17, 1944, President Franklin Delano Roosevelt addressed a letter to Dr. Vannevar Bush, reading in part:

> The Office of Scientific Research and Development, of which you are the Director, represents a unique experiment of teamwork and cooperation in coordinating scientific research and in applying existing scientific knowledge to the solution of the technical problems paramount in war. Its work has been conducted in the utmost secrecy and carried on without public recognition of any kind; but its tangible results can be found in the communiqués coming in from the battlefronts all over the world. Some day the full story of its achievements can be told.
>
> There is, however, no reason why the lessons to be found in this experiment cannot be profitably employed in times of peace. The information, the techniques, and the research experience developed by the Office of Scientific Research and Development and by the thousands of scientists in the universities and in private industry, should be used in the days of peace ahead for the improvement of the national health, the creation of new enterprises bringing new jobs, and the betterment of the national standard of living.

Any consideration of this question brings to mind others. What did our scientists do in previous wars? What organizations did they have, if any, to coordinate whatever it was they did? Did these organizations survive the war for which they were created? What useful role have they played in the development of our scientific institutions? What are the previous wartime accomplishments of American physicists? The present article has been written to answer these questions. It recounts the specific activities of our physical scientists in previous wars, and it also shows how that particular story forms a major part of the whole story of the relations between American scientists and their Government during times of peace and times of war.

The Civil War marks the first coordinated effort of American scientists to serve their country in war time. It also marks the formation of the National Academy of Sciences, created during that war and culminating a long series of attempts to form a truly *national* organization of our scientists. That organization outlived the Civil War; in fact its act of incorporation contained provisions for subsequent peacetime activities, one of the chief of which was to act as the official scientific advisor to the Government. This function decreased in importance as the years passed, through no fault of the Academicians. The fact is that the Government itself set up scientific bureaus to serve the function for which the Academy was created. By the time of the first World War, it became necessary for the Academy to set up a new organization to organize American scientific effort, the National Research Council. This body likewise was organized with an eye to the future and its activities in the peacetime years since 1918 are probably better known to most physicists than are those during the war years. Now, in the midst of another great war, we have still another organization, the Office of Scientific Research and Development. In contrast to its two predecessors, this body contains within its framework of organization no plan to continue itself as a peacetime agency. As

* The writer wishes to express deepest gratitude to Lieutenant Bernard Barber, USNR, with whom he did the original researches out of which this article grew; and to Dr. Henry Guerlac, of the University of Wisconsin, who provided the initial stimulus to the writer's interest in the subject of science and war.

a matter of fact, many physicists would not care to have it function in its present state after the war. A natural question which must arise is: Will every new war that we are engaged in involve the establishment of a new body? This question has probably been answered already in the creation of the Research Board for National Security[1] (composed of Army and Navy officers and civilian scientists) whose formation has been recently announced in the public press.

In one sense, this article may be compared to *Hamlet* presented without the leading character: it contains almost no discussion of the work done by American physicists during the present war. In each major conflict in which this country has been engaged, physicists have done their part—a part that has become increasingly important with each successive war. The Revolution marks the nadir of that activity, and the present war marks its zenith. But that latter part of the story cannot be told until the war is over; in fact, it may never be told, just as the complete story of the World War has never been told.

The fact that physicists have played an ever-increasing role in warfare in our country reflects the growth of physics itself in the world at large and of its prosecution among us. The story likewise shows how science in general has played a greater and greater part in our civilization. The ablest summary of this development was made by the Science Advisory Board in 1934, when it said:

> In the evolution of our national life we have reached a place where science, and the research which has discovered and released its powers, cannot be regarded as matters of accidental growth and application, but must be consciously related to our social life and well-being. What these relations are or may become is now a matter of general or public concern.

The Revolution

Eighteenth-century America produced two physicists of the first rank: Benjamin Franklin and Benjamin Thompson, later Count Rumford. Both were active during the Revolution; one a patriot and the other a Tory. They supported opposite sides. Yet neither one nor the other acted during the struggle as a physicist. Thompson (1753–1814), at first a major in the provincial army, was charged in 1774 with "being unfriendly to the cause of Liberty." He retained his commission until the time when Washington compelled the British troops to evacuate Boston. Thompson was the official bearer of this news to London. He returned to America as a Lieutenant Colonel of George III's American Dragoons and saw action against the American forces in Charleston, South Carolina and New York. Thompson served in a purely military capacity, although he had already completed his training as a physicist under Professor John Winthrop, physicist and astronomer, at Harvard College.[2]

Franklin's activities during the Revolution are well known; as leader of the patriotic forces at home and as minister plenipotentiary at the court of Louis XVI of France he served his country well in the political sphere.[3] Yet, in two very important ways, Franklin's career as physicist was of vital importance to his country. The first way is direct. At the very beginning of the Revolution, the American army was under many technical handicaps: lack of arms and ammunition (especially gunpowder), lack of an organized medical corps, lack of scientifically trained military engineers. Seven days after George Washington had taken his place as "General and Commander in Chief of the United Colonies," he wrote a detailed letter to the "President of the Congress" in which he laid stress upon the "Want of Engineers to construct proper Works and direct the Men." He added: "I can hardly express the Disappointment I have experienced on this Subject." When Franklin arrived in France in December 1776, he immediately made known to the minister of war the desire of Congress to "secure skilled engineers, not exceeding four." These military engineers formed an invaluable part of Washington's general staff, laying out principles of scientific strategy, planning the encampment at Valley Forge, and assisting in the direction of the siege of Yorktown. Their majestic achievements form an as yet unwritten chapter in American history.[4]

[1] "Establishment of Research Board for National Security," a joint statement by Hon. Henry L. Stimson, Secretary of War, Hon. James Forrestal, Secretary of the Navy, and Dr. Frank B. Jewett, President, National Academy of Sciences; a statement by Karl T. Compton, Chairman, Research Board for National Security; and a statement by Frank B. Jewett; Am. Scientist **33**, 112–116 (1945). Includes a list of members of the Research Board for National Security. See also Science **101**, 226 (1945).

[2] Ellis, *Memoirs of Benjamin Thompson, Count Rumford* (American Academy of Arts and Sciences, 1871), pp. 55–150. See also: M. S. Powell, "Count Rumford: soldier, statesman, scientist," Am. J. Phys. **3**, 161 (1935); chapter on Rumford in Jaffe, *Men of science in America* (Simon and Schuster, 1944).

[3] Van Doren, *Benjamin Franklin* (Viking Press, 1938).

[4] I. B. Cohen, "Science and the Revolution," Technology Rev. **47**, 367–368, 374–378 (1945). This subject has been investigated by Dr. Henry Guerlac of the University of Wisconsin, but the important results of his researches have not as yet been published.

The second way in which Franklin's scientific career served his country was indirect. When Franklin went to France he was not an unknown, "provincial" American. He was an internationally famous scientist; he was a Fellow of the Royal Society of London (elected April 1756) and had been the recipient of that society's Copley medal; he was one of the eight foreign associates (associés étrangers) of the Académie Royale des Sciences (Paris). His book on electricity had already been published in ten editions in four languages—five in English, three in French (in two translations), one in German, and one in Italian. The scientific world spoke about electricity in the original terms introduced into that science by Franklin (including plus and minus, or positive and negative), while the public at large knew him as the inventor of the lightning rod, the man who had finally rid humanity of the terror of the lightning stroke.[5] Celebrated as a leading scientific personage and acknowledged to be the world's foremost "electrician," his book "bid fair to be handed down to posterity as expressive of the true principles of electricity; just as the Newtonian philosophy is of the true system of nature in general."[6]

Franklin had given up most of his scientific research in order to serve his nation, but he had done so with conscious intent. As early as 1750 he had written:

> Had Newton been Pilot but of a single common Ship, the finest of his Discoveries would scarce have excus'd, or atton'd for his abandoning the Helm one Hour in Time of Danger; how much less if she carried the Fate of the Commonwealth.

And, although he himself gave up most of his time and energy for "the Commonwealth," he constantly wished that he might arrive at the happy day when he could give up the "world of affairs" and return, as Sir Joseph Banks, President of the Royal Society, urged him, "to . . . Friends . . . & to those studies which rais'd you formerly to a height less elevated perhaps but I am sure more satisfactory."[7]

The only American physicist to work as such for the American army was David Rittenhouse (1732–1796), self-educated astronomer, physicist, and instrument maker. The Revolution needed men of Rittenhouse's ability, and he became engineer to the Committee of Public Safety early in 1775. He supervised the casting of cannon and the manufacture of gunpowder, experimented on rifling action and on musket balls, and acted as a one-man scientific advisory board. To "beat priorities," he inaugurated a campaign of substituting iron for the lead clockweights in and around Philadelphia, and thereby secured a supply of lead for the manufacture of bullets.[8]

Two results of the Revolution on American science were the founding of the United States Military Academy at West Point and the short-lived United States Military Philosophical Society. The Academy was founded specifically to train engineers, and its physics department became one of the best in the country; its engineers were not only trained for war but for peace as well, and the history of engineering in the United States up to the Civil War is largely a history of the activities of its graduates.

The Military Philosophical Society was founded with the whole Army Engineer Corps as a nucleus, and during its ten years of existence came to include as members other personnel of the Army and the Navy and also civilians. Although devoted to "the collecting and disseminating of military science," it was interested in science applied in peace as well as in war, as its seal and certificate of membership show. This organization was the first *national* American scientific society.[9]

From the Revolution to the Civil War

The War of 1812 did not call upon American scientific resources, save in the field of medicine. In the period following, the efforts of American science were directed toward the formation of *national* scientific institutions. Mostly there were failures, but each seemingly abortive effort left its residue of experience for the ones that succeeded it. Cumulatively, the efforts of American scientists came right up to the gate of their ultimate goal: to establish a permanent national

---

[5] I. B. Cohen, *Benjamin Franklin's experiments* (Harvard Univ. Press, 1941), containing the first *American* edition of Franklin's book on electricity; introduction, esp. chaps. i and iv.

[6] *History and present state of electricity* (London, 1775), 3 ed., vol 1, p. 193.

[7] I. B. Cohen, "Benjamin Franklin as scientist and citizen," Am. Scholar **12**, 474–481 (1943).

[8] Barton, *Memoirs of the life of the late David Rittenhouse, LL.D., F.R.S.* (Philadelphia, 1813); C. M. Dowlin, Am. J. Phys. **10**, 79 (1942); also *Dictionary of American biography.*

[9] National, in contrast to other organizations that were founded earlier, such as the American Philosophical Society (Philadelphia) and the American Academy of Arts and Sciences (Boston).

Courtesy Massachusetts Historical Society

Thomas Jefferson's certificate of membership in the United States Military Philosophical Society. The various motifs or emblems show the aims of the society as indicated by the motto, *Scientia in Bello & Pax*.

scientific organization, one closely related to the Government in a double sense—through the obligation on the part of the Government of financial assistance, and through the duty of the scientists of service and official advice.

Plans for a national scientific institution were first laid, during Thomas Jefferson's administration, by Joel Barlow,[10] poet and statesman, who was American Minister in Paris. A bill for the incorporation of an institution to encourage and diffuse scientific knowledge was introduced into the Senate in 1805, but died in committee. The next plan was organized by three who knew Barlow personally and were acquainted with his efforts: Thomas Law, a friend and neighbor; Josiah Meigs, a former college classmate; Edward Cutbush, an acquaintance.[11] These three incorporated the Columbian Institute for the Promotion of Arts and Sciences in 1816 with a small grant from Congress.[12] The original objects were the improvement of agriculture and a survey of natural resources, but the scope was substantially enlarged in 1820 to provide for four "classes" of members "*viz.:* of mathematical sciences, physical sciences, moral and political sciences, and the fine arts." The model was the Institut de France (Académie des Sciences). Lacking funds, which an unwilling Congress refused to provide, the Columbian Institute did not have a long life; yet there were some positive achievements and many influential people came to believe in its credo: "Where genius and talent are respected and promoted, the arts and sciences will flourish and the wealth and power of the nation increase."[13]

While political leaders and scientists were busy trying to decide whether the Government ought to sponsor science in any form, or whether this would be part of the centralization of government that the advocates of states' rights so feared, the Smithsonian bequest was announced. For a reason never fully determined, James Smithson, an Englishman, who died in 1835, bequeathed the sum of about a half million dollars "to the United States of America, to found at Washington, under the name of the Smithsonian Institution, an Establishment for the increase and diffusion of knowledge among men."[14] Soon after, in 1840, the National Institution for the Promotion of Science was organized and the members of the now-defunct Columbian Institute were invited to join it. The guiding spirit, founder and first president was Joel R. Poinsett, then Secretary of War, whose name is celebrated among us today through the beautiful Christmas flower which he introduced from Mexico, the *poinsettia pulcherrima.*

Poinsett[15] introduced many sweeping reforms into the army. He dispatched officers to study at various schools in Europe and to report to him the latest technical developments in the field of military operations. He organized the first American "horse" artillery, or light artillery, based on the Napoleonic pattern. He personally supervised the tests designed to improve the quality of ordnance, and under his direction the Corps of Engineers was expanded and modernized. His skilled engineers did most of the surveying for the railroads to the West, and they brought back Indian relics, and notes on the fauna, flora, geological formations, mineral deposits, and the habits and customs of the Indians.

Poinsett's Institution grew and prospered. It boasted correspondence with 142 learned societies, distributed over Europe, Africa and Asia, and with 47 similar societies in America. There were 350 "resident" and 1250 "corresponding" members. It had the support of many statesmen, of educators, and of scientists, and seemed very likely to be the recipient of the Smithson bequest. President Tyler declared: "Where can it [the Government] find a safer depository for the fruits of its expeditions . . .? What can it better do for the 'increase and diffusion of knowledge among men,' than by patronizing and sustaining this magnificent under-

[10] Article, "Barlow," *Dictionary of American biography.*

[11] For biographies of Cutbush and Law see G. B. Goode, "The genesis of the United States National Museum," *Annual report of the Board of Regents of the Smithsonian Institution . . . for the year ending June 30, 1897.* Much useful information on the history of American science appears in *Report of the U. S. National Museum. Part II* (Government Printing Office, 1901), pp. 83–192, with a separate title page, "A memorial of George Brown Goode, together with a selection of his papers on museums and on the history of science in America."

[12] Rathbun, "The Columbian Institute for the Promotion of Arts and Sciences," Bull. U. S. Nat. Mus. **101** (1917); J. W. Oliver, "America's first attempt to unite the forces of science and government," Sci. Mo. **53**, 253–257 (1941).

[13] E. Cutbush, *An address delivered before the Columbian Institute, for the Promotion of Arts and Sciences, at the City of Washington, on the 11th January, 1817* (Gales and Seaton, 1817).

[14] Smithson. Misc. Coll. **21** (1879–1880); S. P. Langley, "James Smithson," pp. 1–24 of George Brown Goode (ed.), *The Smithsonian Institution* (Washington, 1897).

[15] "Poinsett," *Dictionary of American Biography;* Herbert E. Putnam, *Joel Roberts Poinsett, a political biography* (Mimeoform Press, 1935).

taking?" But Congress thought otherwise and refused to grant it control of the Smithson bequest or any further funds, with the result that the National Institution, like its illustrious predecessor, declined and disbanded.[16]

One positive achievement of the National Institution was its effect on the disposition of the Smithson bequest. Had the Smithsonian Institution been organized before the National Institution, it might well have become a school, an observatory, or an agricultural station. In 1846, however, in the words of George Brown Goode, "the country was prepared to expect it to be a general agency for the advancement of scientific interest of all kinds—as catholic, as unselfish, as universal as the National Institution."[17]

As inaugural Secretary of the Smithsonian Institution,[18] Joseph Henry, the greatest living American physicist, was called from the College of New Jersey, now Princeton University. Henry described his reactions to the proferred appointment as follows:

> Being at the time engaged in a series of original researches, I did not at first entertain the proposition; but afterwards, on the expression of the opinion of the more prominent members of the American Philosophical Society that it was my duty, as the only scientific candidate that had been proposed, to accept the appointment, I accordingly consented, was elected, and entered upon my duty; not, however, without much anxiety and great solicitude as to my ability in the line of administration, but with the hope that the policy which I wished to inaugurate would be readily understood and properly appreciated; that my plans would be immediately adopted; and that, after seeing the Institution fully under way in the direction proposed, I might retire from its charge, return to my former position in the College of New Jersey, and resume my scientific investigations. In this, however, I was sadly disappointed.[19]

Joseph Henry[20] was born of humble parentage in Albany, New York, on December 17, 1797. He attended the Albany Academy and became an engineering surveyor for the state of New York. In 1828 he was elected professor of mathematics and natural philosophy at the Albany Academy and embarked upon a series of epoch-making researches in the comparatively new domain of the relation between electric currents and magnetic fields. He improved the form of the electromagnet and by 1831 had used it in a small telegraph system in his laboratory. Appointed professor at Princeton, he continued his researches and completed his account of the fact that when a long circuit, or one containing a "spiral" conductor, is broken, a bright spark appears at the gap. He explained his discovery by the inductive action of the current on the conductor.

Henry was the first to detect induced currents, and applied the principle to the invention of a small electromagnetic motor. Unfortunately, he did not publish his results, and Faraday, working independently in England, was the first to announce this important discovery. Henry also discovered the induction of a current in a coil surrounded by another coil containing a current and developed the principle of the transformer. His studies of certain types of spark discharge from condensers discharging through coils led him to the belief that these discharges were of an oscillatory nature. But at this point, alas!, he gave up his academic researches for the administrative duties in connection with the Smithsonian Institution.[21]

When Joseph Henry left the laboratory and the classroom at Princeton to become the inaugural Secretary of the Smithsonian Institution he remarked that he was sacrificing "future fame to present reputation." Like Franklin, no sooner had he proved his talent for original research in physics than he was called to duties of administration and the conduct of affairs. And, in words similar to Franklin's, he was heard to remark in later years, "Ten, fifteen, or twenty years ago I made various experiments upon these points, but my duties in Washington have prevented me from pursuing my investigations further." He

---

[16] *Constitution and by-laws of the National Institution for the Promotion of Science, established at Washington, May, 1840* (Gales and Seaton, 1840); *Constitution of the National Institution for the Promotion of Science, established at Washington, May, 1840. Amended and ordered to be printed, April, 1841* (Peter Force, 1841); J. R. Poinsett, *Discourse on the objects and importance of the National Institution* (P. Force, 1841); *Proc. Nat. Inst. Prom. Sci.* (series 1 and 2).

[17] Rep. U. S. Nat. Mus. **109** (1897).

[18] The history of the founding of the Smithsonian Institution may be found in William J. Rhees, "The Smithsonian Institution: documents relative to its origin and history," Smithson. Misc. Coll. **328** (1879), **1085-1086** (1901). See also refs. 11 and 14.

[19] *A memorial of Joseph Henry* (Government Printing Office, 1880). Contains essays by various writers.

[20] "Henry," *Dictionary of American Biography;* essay by J. G. Crowther in *American Men of Science* (Norton, New York, 1937); essay by Jaffe, ref. 2; W. F. Magie, Proc. I.R.E. **30**, 261 (1942); Am. J. Phys. **10**, 335 (1942).

[21] Joseph Henry, in addition to his work in electrical science, investigated problems of solar radiation and the heat of sun spots, as well as capillarity, cohesion of fluids, etc. Sir David Brewster wrote, "The mantle of Franklin has fallen upon the shoulders of Henry."

was in the habit of recalling that Sir Isaac Newton had made no great discoveries after he was appointed Warden of the Mint in 1695.

The greatest tribute to Henry epitomizes his career and his service to the nation. It was made by President Garfield after Henry had died and it begins with an account of the "great loss to science" that occurred when Henry made his choice between administration and research in science. "But," declared Garfield,

> . . . something is due to the millions of Americans outside the circle of science; and the Republic has the right to call on all her children for service. It was needful that the Government should have, here at its capital, a great, luminous-minded, pure-hearted man, to serve as its counselor and friend in matters of science. Such an adviser was never more needed than at the date of Professor Henry's arrival at the capital.[22]

It is certainly true that the development of the Smithsonian Institution followed very closely the plans set forth by Henry in his original "programme" for diffusing and increasing knowledge.

Henry made it clear that the "Institution is not a national establishment, as is frequently supposed, but the establishment of an individual, and is to bear and perpetuate his name." The dream of Barlow, Cutbush, Poinsett, and others of a national scientific institution endowed as the official scientific advisory board to the government was not as yet fulfilled.[23] It was not to be realized until the pressure of the Civil War made its creation possible. As for the need for a national organization of scientists, this was satisfied in 1848 with the founding of the American Association for the Advancement of Science.[24]

The Civil War

If ever any one war was marked off from all its predecessors by the transformation made in the art of warfare itself, it was the Civil War. This transformation was due largely to technological and scientific innovation, a factor that was recognized by the leading powers of the world at the time. Sweden, France, England and Prussia sent military observers to report on the activities of the Union forces; the official Prussian spectator was Graf von Zeppelin, the famous inventor. These observers witnessed the first effective use of the railroad and the telegraph in a great conflict. They also witnessed the use of rifled cannon and gun barrels, telescopic sights, armored (ironclad) warships, rotating turrets for naval rifles, breech-loading repeating small arms, fixed ammunition with conoidal bullets and percussion caps, the machine gun, mobile field hospitals and ambulance service at the front, balloon observation and aerial photography, the submarine, poison gas and liquid fire, the use of concentrated foods, machine-made uniforms and shoes.[25]

The emergence of science as an active instrument in the war came during the first days of fighting, when Thaddeus S. C. Lowe, an enterprising young aeronaut, wanted to organize a balloon corps for army reconnaissance. Lowe enlisted the aid of Joseph Henry, who gave a favorable opinion to Secretary of War Cameron, and a balloon corps was organized which rendered valuable service for some two years—from the Battle of Bull Run to the Battle of Gettysburg.[26]

The Smithsonian Institution was soon drawn into extensive war work. Henry, however, found it hard to hate and could not forget that when Jefferson Davis was serving in Congress he was a regent of the Institution and one of its staunchest supporters, as well as a personal friend. An amusing story is told about Henry and the war. One day he was marched into President Lincoln's office by an army officer who declared: "I told you a month ago Professor Henry is a rebel. Last night at midnight he flashed red lights from the top of his building, signalling to the Secesh. I saw them myself."

"Now you're caught," said Lincoln, turning to Henry, "What have you to say . . . why sentence of death should not immediately be pronounced upon you?" Then he laughed and

[22] Ref. 19, p. 94.

[23] Cf. the words of Alexander Dallas Bache in an address of 1851: "Our country is making such rapid progress in material improvement, that it is impossible for either the legislative or executive departments of our Government to avoid incidentally, if not directly, being involved in the decision of such questions [questions involving scientific knowledge and judgment]. Without specification, it is easy to see that there are few applications of science which do not bear on the interests of commerce and navigation, naval or military concerns, the customs, the light-houses, the public lands, post-offices and post-roads, either directly or remotely. If all examination is refused, the good is confounded with the bad, and the Government may lose a most important advantage. If a decision is left to influence, or to imperfect knowledge, the worst consequences follow."

[24] "A brief history of the Association from its founding in 1848 to 1940," AAAS summarized Proceeding for the period 1934–1940, pp. 1–57.

[25] F. A. Shannon, *The organization and administration of the Union Army 1861–1865* (Arthur H. Clark Co., 1928), pp. 142 ff.; R. Burlingame, *March of the iron men, a social history of union through invention* (Scribners, 1940), pp. 428 ff.

[26] H. F. Stansbury, *Aeronautics in the Union and Confederate Armies* (Johns Hopkins Press, 1941), vol. 1.

explained to the officer that on the previous evening Henry had been experimenting from the tower of the Institution building with a new type of signal flare devised for the use of the army. Lincoln added that he knew the facts of the case since he had been present to observe the effectiveness of the new device.

Some idea of the magnitude of the war work done by Henry and the staff of the Institution may be gained from the *Annual Report* for 1864, wherein he declared:

> As most persons are probably entirely ignorant of the services really rendered to the government by the Institution, I may here state the fact that a large share of my time—all, indeed, which could be spared from official duties—has been devoted for the last four years to investigations required by public exigencies. Within this period several hundred reports, requiring many experiments, and pertaining either to proposals purporting to be of high national importance, or relating to the quality of the multifarious articles offered in fulfillment of legal contracts, have been rendered. The opinion advanced in many of these reports not only cost much valuable time, but also involved grave responsibilities. While, on the one hand, the rejection of a proposition would be in contravention to the high importance claimed for it by its author, on the other the approval of it would perhaps incur the risk of the fruitless expenditure of a large amount of public money . . . .
>
> These facts . . . will be deemed a sufficient answer to those who have seemed disposed to reproach the Institution with the want of a more popular demonstration, but far less useful or efficient aid in the support of the government.

Another physicist whose attention was devoted to matters of war was Alexander Dallas Bache (1806–1867), great-grandson of Benjamin Franklin.[27] Bache was admitted to the United States Military Academy at the unprecedented age of 15 because of his unusual scientific ability, and he was graduated in 1825 at the head of his class. After a short period of service in the Corps of Engineers and a term as assistant professor at West Point, he was elected Professor of Natural Philosophy (that is, physical science) at the University of Pennsylvania. He was elected first president of Girard College in Philadelphia in 1836, and in 1843 became superintendent of the United States Coast Survey. In the latter position, he devised many new instruments and methods, and made magnetic surveys a regular part of the work of the Survey for the first time. Stimulated by the work of Gauss and Weber, he made a series of fundamental magnetic observations and established the first magnetic observatory in the United States at Girard College in 1840.

During the war, Bache planned and organized the defenses of Philadelphia, and was successful in keeping in operation the Coast Survey, which many members of Congress wanted to suspend for the duration. He formulated a policy of extensive coöperation with the Navy, and together they planned the blockade, the Port Royal expedition, and many other feats.

But the project dearest to Bache's heart was the coördination of the scientific activities behind the war effort: to examine and report upon various inventions and devices that might facilitate the operations against the Confederacy. There was afoot a double plan to weld together the scientific activities of the nation in support of the nation; a Permanent Commission was one part, a National Academy of Sciences was the other. The idea of a National Academy was Bache's; the idea of a "permanent commission" originated with Admiral Charles Henry Davis, a friend of Bache's and his former assistant in the Coast Survey.

Charles Henry Davis (1807–1877) was educated at the famous Boston Latin School and then at Harvard for two years, whereupon he was appointed Midshipman in the United States Navy.[28] Between cruises, he continued his studies at Harvard College, especially mathematics under Benjamin Peirce, and astronomy and physics, and received his A.B. degree in 1841, "as of the class of '25." From then on, his career was that of "a man of science and a practical officer, keeping the love of science subordinate to the regular duties of his profession."

In 1842 he was appointed Assistant in the Coast Survey, and in 1849 he became the editor of the *American Ephemeris and Nautical Almanac*. This publication had been established by an act of Congress in July of that year, following continued agitation by Davis, Bache, former President John Quincy Adams (long the friend of

[27] "Commemoration of the life and work of Alexander Dallas Bache and symposium on geomagnetism," Proc. Am. Phil. Soc. **84**, 125–351 (1941); I. B. Cohen, "The one hundredth anniversary of the establishment of the Alexander Dallas Bache Magntic Observatory," Isis **33**, 336–338 (1941); "Bache," *Dictionary of American Biography*.

[28] C. H. Davis, *Life of Charles Henry Davis, Rear Admiral, 1807–1877* (Houghton Mifflin, 1899); "Davis," *Dictionary of American Biography*.

astronomy) and Commander Matthew Fontaine Maury, Superintendent of the Naval Observatory, who was the first to chart the currents of the Atlantic Ocean. During an interim period at sea, Davis translated Gauss's *Theoria motis corporum coelestium*, the publication of which gave to the English-speaking world for the first time the theory of least squares. After a period of command at sea during the first part of the Civil War, he became Chief of the Bureau of Navigation, and after the war was Superintendent of the Naval Observatory until his death, with interruptions as Commander of the Brazilian naval station and Commander of the Norfolk Navy Yard. Truly the career of a man of science in Naval service!

In connection with the dual program of organizing America's scientific resources for war, Davis wrote to his family on February 2, 1863:

> How much have I told you, if anything, about a Permanent Commission or Academy? Bache, Henry, and myself are very busy on this topic, and have made a move which will no doubt result in the Permanent Commission. The Academy is more doubtful . . . .

The Permanent Commission was formed in the same month; to it were to be referred "all questions of science and art" upon which scientific advice might be required, and the commission was empowered to call in any associates it might need to aid in its investigations and inquiries. Although officially organized by the Navy Department, its effects were far-reaching and it rendered service to the Army as well as to the government itself. As a matter of fact, the letter of appointment stated clearly that the scientific experts were to serve as advisers to the Government and not merely to the Navy. In this respect its activities should be compared to those of the Naval Consulting Board during the first World War. The activities of the Permanent Commission reveal that it was constantly in session, testing the new inventions and devices and other proposals with which Washington, then as now, was literally swamped. It's membership consisted of Davis, Henry and Bache.

Once the Commission had been established and had begun operating, Davis decided that this was the time to ask Congress for the incorporation of a National Academy of Sciences "in the name of the leading men of science from different parts of the country." Henry was none too sanguine about its prospects, nor was Bache, who had long been the advocate of an academy. In fact, in his report as President of the National Academy, for the year 1867, Henry remarked: "I must confess that I had no idea that the national legislature, amid the absorbing and responsible duties connected with an intestine war, which threatened the very existence of the Union, would pause in its deliberations to consider such a proposition."

Senate Bill No. 555, "to incorporate a National Academy of Sciences," was introduced on February 21, 1863 and was finally passed on March 3, without discussion, in both houses and signed by President Lincoln.[29] Bache became the first President, which office he held until his death, whereupon he was succeeded by Joseph Henry. The Act of Incorporation consists of three sections, the first two of which contain the list of original members and methods of election and administration; the third, which is the most significant, reads as follows:

> *And be it further enacted*, That the National Academy of Sciences shall hold an annual meeting at such a place in the United States as may be designated, and the Academy shall, whenever called upon by any department of the Government, investigate, examine, experiment, and report upon any subject of science or art, the actual expense of such investigations, examinations, experiments, and reports to be paid from appropriations which may be made for the purpose, but the Academy shall receive no compensation whatever for any services to the Government of the United States.

Bache declared in the first annual report:

> The want of an institution by which the scientific strength of the country may be brought, from time to time, to the aid of the government in guiding action by the knowledge of scientific principles and experiments, has long been felt by the patriotic scientific men of the United States. No Government of Europe has been willing to dispense with a body, under some name, capable of rendering such aid to the government, and in turn of illustrating the country by scientific discovery and by literary culture.

Thus the chief function of the Academy was that of scientific adviser to the government; in the throes of the Civil War, the government had more need of such advice than theretofore, and the three-man "permanent commission" had too much work on its hands to do it efficiently. The

[29] Congressional Globe, 37th Congress, 3rd session, pp. 762, 1121, 1131, 1155, 1181, 1500, 1501.

official historian of the National Academy,[30] writing in 1913, points out:

> This governmental relationship is one of the chief peculiarities of the National Academy. Other scientific organizations were founded whose membership was drawn from all parts of the country, whose scope covered all branches of scientific research, and whose transactions reflected credit on their membership and on American science, but none could claim recognition as the scientific adviser to the Government.

Soon many commissions were in operation. These ranged from committees "On protecting the bottoms of iron vessels," "On magnetic deviation in iron ships," "On experiments on the expansion of steam," "On the preservation of paint on army knapsacks," to one "On the question of tests for the purity of whiskey," set up at the request of the Army, upon the suggestion of its Surgeon General. Some of the Academy's first duties had little if anything to do with the war; for example, projects in connection with the Mint and with international standards of weights and measures, which were highly regarded by President Lincoln. From the time of its inauguration until the present, the Academy presents a splendid record of collaboration with the Government in war and peace. "The Academy started out in the stormy days of the Civil War with the idea and intention of helping the Government. It has helped the Government. Its reports have been accepted, its recommendations have been adopted and the Government has shaped its course in several matters of importance in the light of the counsel which it received from the Academy."

Many of the committees of the Academy, especially those "appointed to consider questions of public policy," were augmented by members of government boards and bureaus as well as by members of various other organizations. No regularity can be found in the number of such committees from year to year. During the first 50 years of the Academy's history (1863–1913), two five-year periods elapsed with no call from either Congress or the Executive Department. More than one-third of all the committees appointed during the same 50-year period were appointed during the first five years (including the Civil War years). And, although the subjects brought to the attention of the Academy covered a wide range, those concerned with matters of physics, chemistry and astronomy have predominated.

In the Academy's first annual report, President Bache declared:

> The members of the National Academy put their time and talents at the disposal of the country in no small or stinted measure, freely, fully, by the binding authority of an oath; asking no compensation therefor but the consciousness of contributing to judicious action by the government on matters of science. The more the wealth of such men can be drawn out from the treasure of their knowledge, the richer will the nation be.

Indeed, the general nature of this richness can be seen from the list of "Papers presented to the Academy, to January 1, 1865," exclusive of committee reports. This list contains 25 entries, of which but three are concerned with practical problems arising out of the war: "On the force of fired gunpowder, and the pressure to which heavy guns are actually subjected in firing," by F. A. P. Barnard; "On materials for combustion in lamps for lighthouses," by Joseph Henry; "Criticism on the forms of ships," by Captain J. Cole (presented by Theodore Strong). The others deal with subjects of fundamental research, including such topics as: "Individuality among animals," "The mathematical theory of quantity," "The Saturnian system," "Shooting stars," "The forms of icebergs," and "The metamorphoses of fish."[30]

Just as science had affected the war, so had the war affected science. The following extraordinary statement of Joseph Henry seems hardly to have been written as long as 80 years ago:

> Although the immediate object of war is the destruction of life and property, yet a state of modern warfare is not a condition of evil unmingled with good. Independent of the political results which may flow from it, scientific truths are frequently developed during its existence of much theoretical as well as of practical importance. The art of destroying life, as well as that of preserving it, calls for the application of scientific principles, and the institution of scientific experiments on a scale of magnitude which would never be attempted in time of peace. New investigations as to the strength of materials, the laws of projectiles, the resistance of fluids, the applications of electricity, light, heat, and chemical action, as well as aerostation are required.
>
> The collection of immense armies of individuals of different ages and nations affords the means of obtaining data of much interest to the ethnologist, while the facts which are gathered from the unusual experience of the battle-field and hospital afford materials for the advance of physiology, surgery, and

[30] F. W. True, *A history of the National Academy of Sciences 1863–1913* (Washington, 1913). The official history.

medicine, which a century of ordinary observation would fail to furnish.[31]

Without question, American science emerged from the Civil War with a new dignity based on service. It had achieved official recognition by the Government, and its place as a vital force within the social commonwealth was firmly established. With its new status, American science assumed new responsibilities. By accepting its duties during the national emergency, American science had fulfilled the manifest destiny chartered by Franklin's immortal words concerning individual scientific effort and the "Fate of the Commonwealth." Joseph Henry declared:

> It is not enough for our government to offer encouragement to the direct promotion of the useful arts through the more or less fortunate efforts of inventors; it is absolutely necessary, if we would advance or even preserve our reputation for true intelligence, that encouragement and facilities should be afforded for devotion to original research in the various branches of human knowledge.[32]

Now that the war was over, American science had the right to expect continued and increased assistance, service, coöperation from the Government. And, in the same measure, the Government, and the society for which it stands, had the right to expect the same from American science.

From the Civil War to the First World War

The progress and organization of American science was an important aspect of the general expansion that occurred in the national life after the Civil War. The nation was bound together by the railroad, the telephone and the telegraph; inventions multiplied as never before; large scale industry became the type rather than the exception—in all of which science participated, both in rendering aid and in deriving advantage.

The "great age" of American higher education dates from this period. Thus was established the indispensable basis for the transmission and the increase of scientific knowledge. Under the terms of the Morrill Act, passed by Congress in 1863, practically all of the states chartered colleges supported by grants of land from the Federal Government. The same year marked the founding of the first of America's higher institutions for training scientists and engineers, The Massachusetts Institute of Technology. The older, privately endowed colleges flourished under the munificence of men who piled up fortunes in meat-packing, in coal-mining, and in railroad operations. Led by the newly established Johns Hopkins University, colleges incorporated graduate schools as a fundamental part of their educational system and organized professional schools in the sciences and in engineering. By the turn of the century, the foundations were adequately laid for the independence of American science. It was no longer necessary to travel to Europe to pursue the higher scientific learning.

The growth of American science in this period is recorded clearly in the membership rolls of the American Association for the Advancement of Science. During the Civil War years, its meetings had been suspended. Once begun again, at Buffalo in 1866, they attracted the ever-increasing number of American scientists. The growth of membership is striking:[24]

Year	*Members*
1860	644
1870	536
1880	1555
1890	1944
1900	1925
1910	7950

This period was one of specialization and pioneering in science. Many scientific societies were founded to cater to special interests rather than to all scientific subjects inclusively. The larger endowments and increased facilities of the universities now allowed scientists to devote all their time to a single subject. Partly a result of the new specialization was the large number of eminent pioneers in so many fields. And scientific societies devoted to but one particular subject were made necessary, and in fact became possible only, as a result of the high degree of scientific specialization.

Despite its great progress, science fell into looser, rather than tighter, connections with the Government. For one thing, its new independence helped to turn it in other directions. Private educational and research institutions gave the scientist ample support in his research. Science prospered independently of the Government because of the beneficences of private capital. Whereas formerly the expense of building and maintaining an observatory had been so great that only the Government appeared able to

[31] *Annual report of the Board of Regents of the Smithsonian Institution for the year 1862*, pp. 13–14.

[32] *Report of the National Academy of Sciences for the year 1867*, p. 1.

undertake it, now there were men such as James Lick with private fortunes large enough to build and endow such an instrument of science. More and more scientists turned away from the direct application of their knowledge to the purposes, plans and needs of the Government. Some devoted themselves to "pure" science and to teaching; others became pioneers in the embryonic industrial research laboratories. If the contemporaneous society suffered from these two trends, it is certain that our age has profited by them. Today we are thankful that our educational institutions are large enough to turn out great numbers of trained scientists and that our industry is constantly making use of the knowledge these men can apply.

Relations between the National Academy of Sciences and the Government became ever more tenuous in this period. There were fewer and fewer Government requests, and when the Academy took the initiative, its recommendations went unnoticed, as in the case of forest conservation.

Members of the Academy did not omit to search their own souls for the cause of this disregard. George Ellery Hale, Foreign Secretary of the Academy, made the most thorough inquiry. He pointed out that the former function of the Academy—to advise on the scientific nature of particular matters—had been assumed by the Government's own scientific agencies, such as the Bureau of Fisheries, the Bureau of Animal Husbandry, the National Bureau of Standards, the expanded Weather Bureau, and so forth. It was now necessary for the Academy to make a new place for itself in the national and the Government life. "It remains true," said Hale, "that questions of broad scope, requiring the coöperation of authorities in several fields of knowledge for their solutions, must arise from time to time. In such cases, the Academy can afford assistance obtainable in no other way, and an enlightened government will advantageously seek its counsel."[33] Shortly afterwards, the beginning of the World War compelled the Government to assign a host of particular problems to the National Academy of Sciences.

Had the Spanish-American War assumed greater proportions, possibly the service of American scientists during the emergency might have restored somewhat the earlier viewpoint. But that war affected few aspects of our normal life. With one exception, scientists rendered but little service. The exception was Samuel Pierpont Langley, Secretary of the Smithsonian Institution.[34]

Langley was primarily an astronomer; his work in astrophysics entitles him to a very high place in the history of science. His major work was the study of radiant heat, and his "bolometer" enabled him to make very many precise and epoch-making measurements. One of his most interesting experiments was in answer to the question: How much energy do we need to produce a sensation of light? He found the answer to be that man could sense a flash of light when it deposited 3×10^{-9} erg on his retina.[35] This discovery achieved significance when, in 1900, Planck's quantum theory yielded the fact that for the frequency of light used by Langley, each quantum has a value of 3.6×10^{-12} erg; therefore the 3×10^{-9} erg found by Langley represents about 1000 quanta. It achieved significance again in 1911 when Einstein formulated his photochemical equivalence law, according to which, in primary photochemical reactions each quantum of energy is absorbed by a single molecule which is then changed chemically by it. Langley's measurement thus means that if a flash of light is to be seen, about 1000 molecules of light-sensitive material in the eye are changed; an astonishingly small chemical change! The most recent investigator in this field, Selig Hecht, writes that this work of Langley's, though incorrect, is "wrong in the right direction!"

> The real value is even smaller than what he found; it is smaller by a factor of 10. This is mainly because Langley worked before the physiology of vision was known sufficiently well to enable him to use the eye most effectively. However, this is of minor importance compared to the establishment of an approximate order of magnitude.[36]

The latter part of Langley's life was devoted to the science of aeronautics. He made studies of the flight of birds and of the various factors which would make mechanical flight possible. Dr. C. G. Abbot, recently retired Secretary of the Smithsonian Institution, has written:

> [The] greatness of his contribution to aviation depends not only on his pioneering laboratory investi-

[33] G. E. Hale, "National academies and the progress of research," Science **38**, 681–698 (1913), **39**, 189–200 (1914), **40**, 907–919 (1914), **41**, 12–22 (1915).

[34] "Langley," *Dictionary of American biography.*

[35] S. P. Langley, "Energy and vision," Phil. Mag. **27**, 1 (1889).

[36] S. Hecht, "Energy and vision," Am. Scientist **32**, 159–177 (1944).

gations and successful long-distance flights of large power-driven models, but on the very fact that a man of his reputation should have adventured it in a field at that time so much ridiculed.[34]

Langley's model No. 5, powered with "petrol-heated flash-boiler steam engines of about five pounds weight per horsepower," was catapulted from a houseboat on the Potomac on May 6, 1896, and flew for about 3000 ft with "excellent stability" until its supply of fuel was exhausted. In November, model No. 6 flew 4200 ft under its own power. At that time Langley said:

> I have brought to a close the portion of the work which seemed to be specially mine—the demonstration of the practicability of mechanical flight—and for the next stage, which is the commercial and practical development of the idea, it is probable that the world may look to others.[34]

Nevertheless, when, with the sanction of Theodore Roosevelt, the War Department asked Langley whether his machine might be used in the war which had just started with Spain, Langley not only replied in the affirmative, but said he would personally undertake its construction. An appropriation of $50,000 was made for this purpose. The machine was completed in 1903, but two attempts at launching it proved so ignominiously unsuccessful that Langley was ridiculed throughout the press and his machine was dubbed "Langley's folly."

(*This is the first of two articles on American physicists at war.*)

Pierre Gassendi and the Revival of Atomism in the Renaissance

R. B. LINDSAY
Brown University, Providence, Rhode Island

First, then, let us honour the atom, so lively, so wise, and so small;
The atomists next let us praise, Epicurus, Lucretius and all.

JAMES CLERK MAXWELL (1874).

TODAY we take atoms for granted. The value of the atomic concept for modern physics is so great that no scientist ever gets excited any more over the question of the reality of atoms. But this attitude of detachment has not characterized every stage in the evolution of science, and there were epochs when it meant a great deal whether one believed in atoms or not, and, more particularly, whether one stated this belief in public. This was peculiarly the case in the late Renaissance, the period to which the subject of the present paper belongs.

But first let us remind ourselves that atomism as a philosophy is very old. We recall the Greek philosophic school represented by the rather shadowy figures of Leucippus and Democritus of the fifth century B.C., best known to us by the adoption of their philosophy by Epicurus (341–270 B.C.), who taught it at Athens along with a lot of other things. The best summary of the atomic point of view in antiquity is provided in the poem of Lucretius (99–55 B.C.), *De rerum natura*. Unfortunately for atomism, Aristotle, who "invented" physics and wrote the first serious textbook on the subject, did not believe in atoms. He realized quite correctly that belief in their existence involves the notion of empty space or perfect vacuum; and he was quite sure that there is no such thing as a vacuum. We shall not stop to recapitulate his argument, which is commonly used as one horrible example among others of the pernicious influence which he exerted on the development of physics. Aristotle threw the emphasis of his teaching on the *continuum* point of view, the theory that matter has a completely continuous structure and that any so-called piece of material is indefinitely subdivisible.

It would be tedious to try to follow in detail the varying fortunes of the two fundamental viewpoints, the atomic and that of the continuum, through the centuries from the decline of classical Greek philosophy to the Middle Ages. It is sufficient to note that the continuum triumphed and the atomic theory went into hiding. The philosophy of Plato and Aristotle found favor

with the early fathers of the Christian Church. When the latter considered atomism at all, it was to stress what they felt to be its essential atheism. Epicurus had taught that in the beginning there existed merely atoms and a void, and that the universe had originated in the chance concretion of atoms flying around in quite random fashion; there seemed little room for God and the creation in this picture, and the resulting excoriation of Epicurus and atomism was only to be expected. Atomism definitely became heresy.

Although it is undoubtedly historically dangerous to make generalizations about the Middle Ages, there seems little reason to question the retarding effect of the ecclesiastical prohibition of atomic theory on the development of science. Certainly the emphasis which the early atomists placed on motion as the most fundamental of physical phenomena should have led to an earlier revision of the Aristotelian theory of motion than was actually the case. The dead hand of scholastic philosophy lay like a heavy weight on mechanics until the very end of the 16th century, when Galileo carried out his epoch-making experiments and began to look upon motion from a wholly different viewpoint. We all know the great success he had in laying the foundations of the modern theory of mechanics. It is a striking fact that though Galileo experimented tentatively with the atomic idea in the attempt to account for the properties of matter, he never seemed quite satisfied with the results. The reason he himself gives is that he would not teach seriously what he could not demonstrate mathematically. It is at least equally probable that his difficulties with the church deterred him from pursuing too far a doctrine which was so easily associated with heresy.

It is of course true that the notion of atom never really died out during the Middle Ages. The German Cardinal Nicholas Cusanus (1401–1464) resurrected the term in his philosophical writings of the 15th century, but this apparently had little influence on his contemporaries and successors. Then, in the 16th century, the Italian Giordano Bruno introduced the atomic notion in a semimystical mixture of mathematics and physics but never applied it to the description of physical phenomena. Bruno's end at the hands of the Inquisition was not a pleasant one! Francis Bacon (1561–1626) revived the atomic theory of the Greeks, only to disagree with it. In Germany, the physician Daniel Sennert (1572–1637) introduced a corpuscular theory (1619) for the explanation of chemical processes, and the Italian Sebastian Basso, another physician practicing in Paris, published in 1621 his 12 books of natural philosophy against Aristotle. Significantly, the publication was in Geneva, not Paris.

Basso's work was undoubtedly largely responsible for an interesting attempt to bring the atomic point of view publicly to the attention of the citizens of the French capital. Three bold spirits, Jean Bitault, Antoine Villon, and Étienne de Claves, announced that they proposed on the 24th and 25th of August 1624 to discuss publicly certain theses against Aristotle. It was their intention to hold this disputation with experimental demonstrations in the palace of the late Queen Margaret, former wife of Henry IV. The 14th thesis came out openly in favor of the atomic theory of Democritus and Epicurus and castigated old Aristotle with some severity. About 1000 persons came to see the show, but the performance failed at the last moment to come off, for the authorities, evidently sensing the possibility of a riot, forbade it and arrested de Claves. Villon, the soldier-philosopher as he was called, saved himself only by flight. Nothing is known about what happened to Bitault. The three would-be perpetrators of the crime against Aristotle were exiled from Paris. It was made a crime punishable by death to publish such theses anywhere in the kingdom, or indeed any other similar disputations without the consent of the theological faculty of the University of Paris.

Evidently it was necessary for those who were enamored of atomism in the early 17th century to proceed with some caution! Even the celebrated Descartes, though he introduced a corpuscular hypothesis into his principles of physics, did not go all the way toward the ancient atomistic theory of Epicurus, since he did not admit the idea of vacuum into his theory. And we may also be permitted to recall that just as Descartes' famous treatise *De monde* was on the point of publication in 1634 the news came to him of Galileo's persecution by the Inquisition and the famous abjuration of June 1633. He decided not to publish and kept to his decision until the powerful influence of Cardinal Richelieu was exercised in his behalf; only then did it become safe to proceed. Descartes was no fool and knew his politics.

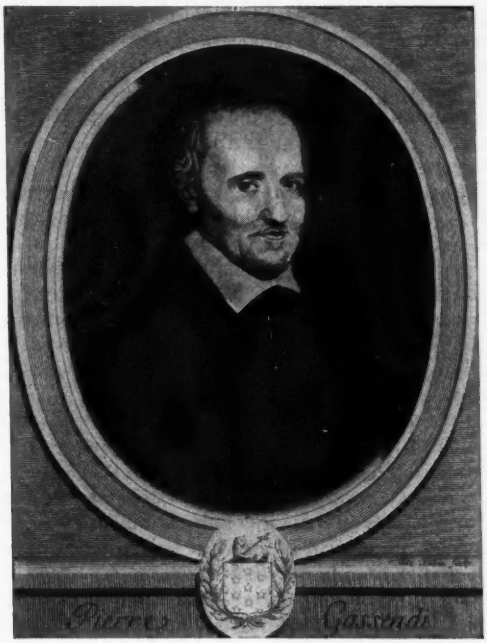
The preceding remarks set the stage for what I cannot help thinking is one of the minor mysteries of the history of science. This is the fact that the

chief reviver of Greek atomism in the late Renaissance, the man whose work is chiefly responsible for the inspiration of Boyle and through him of Newton and hence the whole Newtonian school, was a devout priest of the Roman Catholic Church. He was by no means one of the giants of physical science. His contributions to physics pale into insignificance compared with those of Galileo, Huygens and Newton, but his stubborn determination to breathe new life into the doctrines of Epicurus at a time when it was unfashionable if not positively dangerous to do so provides a fitting reason for devoting some attention to his career and his ideas, the more so as he has suffered rather serious neglect at the hands of the historians of physical science. His name was Pierre Gassendi, and he lived from 1592 to 1655. It is my intention to devote the remainder of this paper to him and his significance in the revival of atomism in the Renaissance.

Outline of Gassendi's Life

At the outset it is only fair to express a certain feeling of disconcertedness if not downright frustration in the attempt to find out what manner of man Gassendi was, his life having been so completely devoted to religion and scholarship that even the most ardent biographer can find little of excitement in it. Confronted by a situation of this sort the student is apt to take one of two ways out: he will either reconstruct the historical environment and place his hero in it by the use of judicious fictional devices, or he will immerse himself in the writings of the man and build a picture of him from what he had to say. The first course can be followed with safety only by the professional historian. The second is not made any easier from the fact that Gassendi was a prolific writer and his six large tomes of medieval Latin form a formidable barrier to a physicist. Apparently no complete translation of his works into any language has ever been made, though a French abridgement was brought out by his pupil Bernier in 1678. The German philosophical commentators appear to be the principal authorities for the significance of his philosophical and scientific writings.

The bare facts of Gassendi's life can be rather briefly stated. He was born of peasant stock in the village of Champtercier in Provence, some four miles from the town of Digne (a town that figured not so long ago in the news of the American invasion of southern France). This was on January 22, 1592. He was baptized plain Pierre Gassend and only later assumed the form of the Latin genitive, presumably to add dignity and style. Starry portents were said to have occurred at the time of his birth, presaging his later scholarly attainments. Like his more celebrated compatriot, Blaise Pascal, destined to be born some 30 years later, little Gassendi was a child prodigy, and the story goes that at the tender age of four he preached sermons out of the window to the astonished edification of the passers by. Sent by proud parents to the college of Digne when hardly out of his swaddling clothes, he was there known as Gassendulus. In 1609 he had exhausted the well of knowledge at Digne and moved on to Aix where he studied philosophy under Fésage, who is supposed to have remarked: "I don't know whether he is my pupil or my master." One would have wished to catch the tone of his voice as he said this! At the mature age of 20 Gassendi became principal of the college of Digne and remained in this position for three



Reproduction of the fine engraved portrait of Pierre Gassendi executed by Lubin. The original is in the splendid collection made by Charles Perrault and published in Paris under the title, *Les hommes illustres qui ont paru en France pendant ce siècle* (1696).

years. At 22 he became a doctor at Avignon and two years later entered the priesthood on the advice of influential friends who may have felt he could take better advantage of his intellectual talents in this way. When the chairs of theology and of philosophy at the University of Aix fell vacant, Gassendi won both of them by the disputation method but graciously turned over that of theology to his old teacher, retaining that of philosophy for himself. His choice reflected to a great extent his future interests and attainments. Indeed one gets the distinct impression that Gassendi looked upon the Church as a bread-and-butter affair—his real heart was in philosophy, which of course in his time meant science or what passed for such.

In the winter of 1624–25 we find the still youthful philosopher in Paris where his reputation had preceded him and gained him the esteem of Descartes and the notable circle of savants who were the precursors of the famous Academy of Sciences. In 1624 Gassendi had already published his first book, *Exercitationes paradoxicae adversus Aristoteleos*. After what had happened in Paris in the summer of 1624 it would seem to have required considerable courage to come out against Aristotle and one wonders how Gassendi got away with it. All through his life he appears to have just coasted along on the edge of heresy. Perhaps in the little town of Digne persons might say things about Aristotle that the venerable faculty of Paris would not tolerate in the metropolis. At any rate, it is also true that by 1625 Gassendi had adopted the Copernican theory and had written to Galileo congratulating him on his defense of it. Obviously the condemnation of Galileo by the Inquisition in 1633 came as a considerable shock. Gassendi weathered the storm by going about his business, thinking his own thoughts but keeping his mouth shut.

Gassendi's career in the church involved much ecclesiastical politics and even considerable litigation. For example, his provostship or deanship (as we might term it) at Digne was secured only after a law suit which lasted seven years. He usually won in such encounters. His biographers lay great stress on what a modest man he was, but he evidently got pretty much what he wanted. In 1645, through the influence of Louis-Alphonse Du Plessis de Richelieu, Cardinal Archbishop of Lyon and Grand Almoner of France and brother of the more famous Armand, Gassendi received the appointment as Royal Professor of Mathematics at the Royal College in Paris, now the Collège de France. Here he joined actively the group of philosophers who met more or less regularly in the cell of the Minorite Mersenne, a distinguished circle that included the mathematicians Fermat, Desargues, Roberval, as well as the tortured genius Pascal. Here the southern philosopher indulged his interest in experimental science. That he was no mean experimenter is shown by his observation of the transit of Mercury across the sun's disk in 1631, according to some authorities the first time it was ever definitely observed by anyone. Gassendi was also interested in acoustics and made measurements of the velocity of sound in air as early as 1635. The value that he found was too high, as Mersenne showed by a more accurate determination. Gassendi did establish one important point, namely, that the velocity of sound in air is independent of the frequency—a fact of which Aristotle was blissfully ignorant. Of course Gassendi made his mistakes also: he believed that the wind has no effect on the velocity of sound. He was wrong here, as any sound ranger in the field artillery knows to his cost.

It was during the professional sojourn in Paris that Gassendi finally brought out his life of the Greek philosopher Epicurus. For many years he had been leaning toward the atomic doctrine and yearned to rehabilitate the tarnished reputation of the ancient atomist. He had long hesitated to come out in the open, however, and for the most part his views had been expressed in letters to his contemporaries, particularly Mersenne. Once again we can only marvel how he finally secured ecclesiastical approval for the publication of the life and philosophy of the hated and despised atomist. It may be that the Church authorities were satisfied that he had succeeded in adapting the Epicurean atomic doctrine to orthodox Christianity. Certainly Gassendi always showed himself a very cautious writer, very different in this respect from Galileo, who liked nothing better than to poke fun at his adversaries. Voltaire later lampooned the theological philosopher with the phrase: "the unsettled, uncertain Gassendi."

In 1648 at the peak of his fame, Gassendi's health broke down and he returned to his ecclesiastical position at Digne. While here he received a pressing invitation from Queen Christine of Sweden to come to Stockholm to take the place of Descartes, who had just died there. Gassendi may have heard of the troubles of his distinguished contemporary in trying to teach the boisterous queen the elements of natural philosophy; he

wrote her majesty a letter emphasizing what a fine girl she was but indicating very politely the impossibility of his acceptance.

In 1653 Gassendi was back in Paris, this time never to return to his old home. In 1654 his physical condition grew steadily worse, and on October 24, 1655 he died at the comparatively early age of 62. The immediate cause of death was doubtless a series of bleedings which his weakened condition could not stand. He was such an abstemious man that it is almost possible to believe he died of starvation. He is supposed to have been buried in the Church of Saint-Nicolas-des-Champs, but no trace of his tomb now remains. Paris was too turbulent a city to die in: he should have died in Digne!

Gassendi's Atomism

We now approach the more technical problem of Gassendi's atomism. Just what did he believe about atoms? The most elaborate and presumably definitive exposition of his ideas is to be found in his *Opera omnia*, published in six volumes in 1658 in Lyon. A reprint was brought out in Florence in 1727. The first volume contains most of his physical philosophy. It is scarcely necessary to say that it is not at all easy reading. The good old Latin words such as *qualitas*, *facultas*, *atomus*, *calor*, *frigor*, *plenum*, *solidum*, *velocitas*, *levitas*, *gravitas*, *densitas*, *raritas*, . . . , are all there; the great question is: what do they mean? Every examination of such a work must convince the physicist of the tremendous difficulties confronting the historian of science whose task it is to penetrate behind the abstract language of the ancient writer to the operational meaning and then to translate into the equivalent scientific terminology of today. This problem is not so difficult in the case of those authors who describe actual experiments and give diagrams or pictures illustrating what they did and what they found. From this point of view Galileo, for example, in much of his writing is open to little or no misinterpretation. When he says he allowed a ball to roll down an inclined plane and draws a picture of the plane, you have no doubt what must have been going on in his head. This is particularly the case when he puts his results in mathematical form, for mathematics is a universal language. Unfortunately, with Pierre Gassendi the case is different. He was evidently writing for his own time and assumed with reason that his contemporaries would have no trouble understanding his language. Though in his exposition of atomic theory he refers frequently to ordinary human experiences and introduces some interesting, if at times far-fetched analogies, he describes next to no experiments, uses no diagrams and presents practically no mathematical discussion. This is indeed a bit strange since his astronomical writings show him to have possessed an adequate grasp of geometry.

A detailed analysis of Gassendi's philosophy is out of place here. We shall try to do justice to the basic ideas particularly as they relate to atomism. In order to appreciate his theory it must first be emphasized that Gassendi broke away from the scholastic philosophy in postulating the existence of a real world of physical objects, existing in space and time, which latter are likewise real entities independent of physical bodies. For Gassendi, space is real but incorporeal; it is unmeasurable, infinite, and eternal. It was so, long before God created the world and will exist forever even if God destroys the world in the meantime. It is absolutely immovable. Wholly independent of God, it was never created; in fact it is uncreatable and indestructible: it just *is!* This space is, so to speak, the frame of reference in which all physical objects are imbedded and all physical phenomena are observed. The same attributes of necessity, incorporeality, and infinite extent are given likewise to time, the medium in which physical events take place. It is flowing or moving extension in distinction to static extension.

Now physical events in space and time form an ever-changing sequence or flux. Gassendi cannot believe this is typical of real existence. There must be a fundamental unchangeable something at the basis of all our perceptions. This something is the *principium materiale* or *prima materia:* prime, or first, matter. It is the stuff of which all perceived objects are made. We have here a reversion to the ancient Greek doctrine of the "principles" and a revulsion from the scholastic philosophy of substantial and accidental forms in accordance with which a jug is a jug because it has the substantial form of "jugginess," while it is light or heavy according as it has the accidental form of lightness or heaviness, and so on.

The question now is: what is the prime matter? Gassendi at once plumps for the atom to serve in this capacity. And what does he mean by an atom? This is important, since the notion of particle or corpuscle was fairly common in his time but must not be confused with Gassendi's

atom. To him the atoms are the final and smallest components of all physical bodies. They are not mathematical points but have finite extension, even if too small to be observed directly by the senses. They are so solid that no natural force is able to break them. Gassendi admits that all actually observed objects can be subdivided into smaller pieces. This seems obvious. But he deliberately refuses to postulate the possibility of the indefinite extension of this process. Mathematically it may be imagined, but physically it is impossible. To believe in its possibility is, according to Gassendi, to nullify the fundamental axiom of all scientific reasoning: from nothing comes nothing, and nothing cannot be converted into something. In our modern sophistication we may well admit that all this is arguing with meaningless words. But perhaps it was a good thing for the development of physics that there was a time when some one took it seriously.

Gassendi assumed that all the substance in the world is located in the atoms; everything else is empty space. In other words, he believed firmly in the existence of a vacuum. This is interesting since it was during Gassendi's lifetime that Torricelli invented the barometer and made his discovery of atmospheric pressure, and Pascal suggested the famous confirmation of this result through the observation of the fall of the barometer with increase of altitude. Pascal apparently had no doubt that the space above the mercury surface in the barometer tube is a genuine vacuum. One might suppose that this would have been a deciding influence on the philosopher of Digne. This does not appear, however, to have been the case. Actually Gassendi did not believe in the Torricellian vacuum. His vacuum was a hypothetical one assumed for the purpose of providing empty space for the atoms to move around in.

The most important property of atoms is their mobility or tendency to motion, called by Gassendi *gravitas*. It is true that their size and shape are also significant and these three properties must be sufficient to account for all the diversity of observed phenomena. The variety of possible sizes and shapes is very large, but in contradiction to the ancient atomists is not infinite. Like a wise man, Gassendi dodged infinity; he must have realized that too many clever people had burned their fingers with this tricky concept. Large enough, but not indefinitely so, seems to have been his motto. Arguing for atoms of differing shapes, Gassendi points to the various kinds of crystals observed in nature: what more natural than to assume, for example, that the atoms making up common salt are cubical in shape since the salt crystal takes the cubical form? On the other hand, certain atoms may be of very irregular shape, even to the point of having hooks on them by which they can attach themselves to other atoms and hence form groups with special properties—molecules, as we now call them.

When God created the atoms (for Gassendi is careful to give the Creator the credit for being the first cause of things, though he usually disregards Him thereafter), He endowed them with an urge to motion which is the same for all atoms and which they can never lose. This mobility results in actual motion with a high velocity (precisely how high is not indicated) in a vacuum. In space occupied by other atoms there is continual bumping and atoms may be retarded. It is assumed, however, that while in motion the atom always has the same velocity. Its average velocity may indeed be less than the standard vacuum value because of rest periods due to collisions. Gassendi does not appear to be worried over the fact that this involves infinite deceleration and acceleration. Unconsciously he was getting involved with the wicked demon infinity in spite of himself. As a matter of fact his concepts of motion are very vague. He made no attempt to formulate them mathematically, and if he really read Galileo's expositions on mechanics, certainly he either did not understand them or did not choose to follow them. He here missed a golden opportunity: he might have created a full-fledged kinetic theory of matter if he had applied Galileo's principles of motion to his atoms and had worked out the consequences. However, this task was left for Boyle to suggest and Daniel Bernoulli to carry out about a century later.

Gassendi's view of the observed properties, or qualities, of bodies is essentially that of empiricism: he holds that all knowledge of such things comes from our sense perceptions. But it is the task of the philosopher to describe these in terms of a postulated set of constructs. Such a set is provided by the atoms, which by their size, shape, number, arrangement, and motion are responsible for all observed phenomena. A few illustrations must suffice to show how the concept was applied. One of the most obvious differences between substances is their density. We say a block of lead of given volume is more massive than a block of wood of the same volume: the

density of lead exceeds that of wood. Why is this? Gassendi has an easy answer: in the wood there are relatively few atoms in the given space; in the lead there are relatively many atoms in the same space. Or, to put it another way, there is more vacuum, or empty space, in the less dense substance. Density is thus reduced to a numerical property, and apparently no attention was paid to the possible difference in mass of the constituent atoms. The physicists and chemists of the 19th century could therefore scarcely avoid considering Gassendi's view as absurd. Curiously enough, with the advent of nuclear physics and the assumption that all atoms are composed essentially of three fundamental building blocks—protons, neutrons, and electrons—we are in a sense re-establishing the numerical criterion. But it is very dangerous to indulge in the game of reading the specific scientific ideas of the present into those of the past, and I shall refrain from this pastime, merely remarking that there appears to be nothing in Gassendi's philosophy corresponding to the interaction between atomic particles in our modern theory.

Some materials such as glass are transparent. Others like metals are opaque. Why is this? The explanation of Gassendi is simplicity itself. In the transparent body there are more vacant spaces between the atoms for the light particles to go through; these vacua, or pores, are present in fewer numbers or absent altogether in opaque bodies. Notice that Gassendi talks about light in terms of particles. He was very thoroughgoing in his atomism. Light, heat, cold, sound are to him all atomic phenomena.

Thus there exist certain very small, round and fast-moving atoms which can be emitted by certain substances, for example, flames. These are able to penetrate the pores of other substances and push apart their constituent atoms. This process constitutes heating, with the concomitant change of size of the heated body. Not content with this not too unreasonable application of his theory, Gassendi proceeds to explain the nature of cold by another set of atoms, which are much larger and slower than the heat atoms. Moreover, they are of tetrahedral form and when they penetrate into bodies have a tendency to unite the constituent atoms. We should say that this explanation is less plausible, violating as it does the fundamental principle of economy in scientific thought. From our modern point of view Gassendi worked with too many different types of atom.

Another illustration is provided by acoustics, for sound is considered to be due to characteristic atoms emitted by the sound-producing body. When these enter the ear they react with a particular set of atoms there to produce the sensation of audition. The velocity of sound is simply the speed of the sonic atoms, whereas the pitch is proportional to the number emitted per unit time, in analogy with the notion of frequency in the wave theory. It has already been mentioned that Gassendi was interested in acoustics and made several experimental observations on sound. He had the correct explanation of the echo as resulting from the reflection of sound from surrounding surfaces, though this of course had been suggested in ancient times.

Any theory of matter and mechanics is bound to account somehow for the fall of objects toward the earth. It must be admitted that this gave Gassendi considerable trouble. His fundamental philosophy forbade him to consider free fall as due to some inner quality of bodies by which they were attracted to the earth—this was too *ad hoc*. The fall must somehow be associated with action taking place directly in the atoms of the falling object. But this action must not be action at a distance; it must be immediate and through direct contact. Hence Gassendi was forced to assume that the earth emits streams of what might be called "gravitation particles" so effectively continuous in character that they possess the rigidity of stretched strings and act like tentacles to grasp the atoms of the falling body and pull them to earth. Since these streams of particles emanate radially, their gripping power decreases with the distance. Just how these particles bring about the increase in velocity of fall which we call the acceleration due to gravity is not clear: here we miss the clear-cut pragmatism of Galileo, who did not pretend to explain *why* bodies fall but succeeded at any rate in describing *how* they fall.

Gassendi's Place in Physical Science

It would be tedious to protract this summary of Gassendi's atomic theory. One cannot help being impressed by its naïveté. As a direct contributor to the advancement of modern atomic physics, the philosopher of Digne missed the boat. Why then do we stop to pay him a moment's consideration? For the simple reason that it is not always the intellectual genius who is wholly responsible for the direction taken by human thinking, though he naturally gets the credit.

Often a man with much humbler attainments by persistent effort can force the attention of his contemporaries on a useful notion even if he himself makes no outstanding discovery thereby. This was the case, I think, with Pierre Gassendi. In resurrecting the heretical atomism of ancient times, he made it fashionable and clothed it in the habiliments of ecclesiastical orthodoxy. He thus made it possible for sincerely religious philosophers such as Boyle and Newton to accept the atomic theory and apply it in their much more significant speculations. The reader of Boyle, in particular, cannot fail to be impressed by the frequency with which the influence of Gassendi is acknowledged and by the warm welcome extended by the famous skeptical chemist to Gassendi's attempt to reconcile religious teleology with Epicurean atomism. To us today this may appear a very trivial matter. But in the 17th century it was a very serious one. Many of the foremost natural philosophers were deeply religious men, both Protestant and Catholic, and were much concerned lest their scientific theorizing should conflict with their religious tenets. From this standpoint it would be unjust to underestimate the contribution of Gassendi.

I am well aware that many physical scientists and possibly philosophers will not endorse the attention which this paper has paid to a man like Gassendi. He certainly was not in the Nobel prize winner class. I still believe, however, that the course of atomic theory would have been harder and more tortuous had he not lived, lectured, and written. Doubtless he was only a humble torchbearer in the procession of science, but the torch he carried was sufficient to produce a light that, however dim, has still been able to penetrate to our time.

The author wishes to acknowledge his indebtedness to his wife, Rachel T. Lindsay, for assistance in the translation and interpretation of Gassendi's writings, and to Professor E. C. Watson, of the California Institute of Technology, for the use of the portrait of Gassendi which accompanies this article.

Brief Bibliography

Louis Andrieux, *Pierre Gassendi.* (Thèse pour le doctorat ès lettres présentée à la faculté des lettres de l'Université de Paris, Paris, 1927.)

Pierre Gassendi, *Opera Omnia* (Florence, 1727). Volume I contains the physical philosophy, including the ideas on atomism.

Kurd Lasswitz, *Geschichte der Atomistik vom Mittelalter bis Newton* (Two volumes, Hamburg and Leipzig, 1890). The material on Gassendi appears in Vol. II, pp. 126–188.

Paul Pendzig, *Pierre Gassendis Metaphysik und ihr Verhältnis zur scholastischen Philosophie* (Bonn, 1908).

Physics and the History of Ideas

Wayne B. Denny
Oberlin College, Oberlin, Ohio

RECENT emphasis upon scientific and technological training in our institutions of higher learning has given rise to several quite different attitudes concerning the role of instruction in the natural sciences within the framework of the so-called liberal arts college. Certain of these positions are worth examining. The first—if, indeed, it can properly be called a position—is held by many science teachers themselves. These teachers apparently assume that *the values of scientific training are so self-evident and so universally accepted that there is no need to examine those values or to defend them.* Many men of science are so convinced of the values inherent in the study of their particular fields that it does not occur to them that they should ever be called upon to justify their profession as a legitimate *liberal educational enterprise.* Or, they may rely upon the popular faith in what is called "scientific" to guarantee them intellectual, educational, and even financial integrity. Or again, they may be too busy with their balances and their measuring rods to give the matter more than casual notice.

In contrast with this uncritical view is a second attitude concerning the role of science instruction in the liberal arts college. This is the position according to which *the study of natural science has no proper role in an educational program devoted to the truly liberal arts.* Now this is not the violently

antiscientific attitude that it appears to be at first. Indeed, it is actually held by some science instructors who have no real sympathy with liberal arts programs as they know them. These men feel that the scientific enterprise can be carried out most effectively in technical schools more wholeheartedly devoted to the scientific and technological ideals. Moreover, this divorce of science from the liberal arts is also advocated by some conservative elements within the arts college—by men whose main intellectual interests are external to the scientific enterprise. Here again, the attitude is not necessarily antiscientific. Rather, these individuals conceive science primarily—if not exclusively—as technological. They would relegate the study of science to special schools because they believe that the aims of technical training differ so much from the aims of training in the liberal arts that no valid unity of purpose could possibly dominate both endeavors within the compass of a single educational organization.

If each of the views just set forth is widely held —and the evidence seems to indicate that this is the case—there can be only one reason. This situation is but the natural consequence of the isolation borne of specialization by those who hold the views. It is but the result of a rigid departmentalism which characterizes the professional education of scientist and non-scientist alike. While we may deplore such narrowness on the part of those who should possess the widest vision, there is evidence that this tendency toward isolation continues to exist, to the serious detriment of liberal education, notwithstanding some heroic attempts to combat it. Within the liberal arts college this is the more unfortunate because the unique value of the so-called liberal arts college is its potential ability to provide a unified educational program—a goal not possible in technical schools.

A unified program of general higher education is not one that offers instruction in science *or* instruction in the humanities and social sciences. Nor is it just a program that offers instruction in science *plus* instruction in the humanities *plus* instruction in the social sciences. This much it must have—yes; but a series of disparate courses taken in isolation is not unity but mere accumulation. Rather, unification implies *one program* providing for the student a background which draws from all fields in the humanities, the sciences and the social sciences—a program which might well include all or most of the work of the first four college terms, preceding more specialized work. Its synthesis is achieved by a frank and even deliberate recognition by the teacher and his students of those valid and significant relations that exist among the various branches of learning at their present stages of development.

But a word of caution is desirable. While such a program deliberately recognizes similarities of method and procedure where such similarities exist, it must not falsely oversimplify the academic picture by suggesting that the methods and aims of all intellectual inquiry form a consistent and integrated whole, for this is not now the case. It was just this attempt at idealistic oversimplification which characterized much philosophy and science at the turn of the century and which eventually discredited them. More recent attempts to impose arbitrary metaphysical systems into the various branches of learning are open to the same objections. They do appear to give the educational program a certain coherence but such artificial coherence is long since out of date. Rather, what is here termed unity in education will show the student where methods, procedures and ways of thinking are not interchangeable from one field to another and so guard him against sophomoric errors. And it will give him some insight into the many problems yet to be solved and, perhaps, stimulate him to act upon them. To the extent that the liberal arts college fails in this regard, the student who seeks a general education is shortchanged and is, perhaps, entitled to a partial refund of his tuition.

If political, territorial and cultural isolation have no place in the modern world, so, too, the intellectual isolation of narrow departmentalism has no place within the modern liberal arts college. If this be granted, then the removal of the barriers which foster that isolation is a major problem for current liberal arts education.

Physics and the Unified Educational Program

What aspects of this general problem are of immediate concern to the physicist? Where does physics stand in relation to the other areas comprising the usual curriculum of the college of arts and sciences? In what ways does the subject matter of physics place demands on the teacher which are different from those placed on teachers in other fields?

In the first place it must be freely admitted that physicists are responsible in some measure for the kind of isolation mentioned earlier and

that their responsibility has been of the same order of magnitude as that of their colleagues in other fields. They have, in many instances, emphasized the technical and technological aspects of physics to the near exclusion of its relations to other areas, a circumstance due in no small measure to the specialization which characterized their own training.

That physicists must share this responsibility is particularly unfortunate because of the relative maturity of physics, which should place the discipline in a favored position to combat isolation. Indeed, of all the sciences, physics is unique because throughout the history of ideas so many of its concepts and ways of thinking have, after modification, found their way into other fields. This situation is obscured somewhat because in so many instances concepts from physics were first appropriated by philosophers who adapted them for their own peculiar purposes and then passed them on to colleagues in other fields. And the process also went on in reverse, particularly during the more tender years of physics. The results of this lease-lend arrangement were occasionally fruitful, often inadequate, sometimes amusing, and in many cases they actually hindered the development of our Western learning. Alas, not all philosophers have been competent physicists, and physical concepts suffered much at their hands. For example, their interpretations of such notions as *particle*, *quantum*, *action* and *indeterminacy* often reveal considerable misunderstanding. Yet, for better or worse, this interchange of ways of thinking, together with the prejudices it produces, constitutes an important chapter in the history of ideas.

But another and more important manner in which the physical sciences have influenced the course of intellectual development is their influence upon language. This influence has long been recognized but it was only recently that its true significance has begun to be appreciated. While it is obvious that the sciences have introduced a host of terms into the common usage of natural language, a more significant contribution has been their influence upon not the terms but the very structure of language itself. It is becoming apparent that the structure of a language must be adapted to the ideas which it is to represent. The arrangement of words and symbols must bear a discernible relationship to that which they purport to represent and, conversely, the ideas expressed are limited by the structure of the language in which they are stated. If the grammar prohibits the adequate representation of new ideas, a new language with a new grammatical structure is made necessary. Physicists were among the first to recognize the limitations of language, and they did not hesitate to develop new mathematical languages and symbolic mediums with their own peculiar rules of syntax better adapted to the business at hand. An obvious example is the invention of the calculus, whose syntax allowed the notions of differentiation and integration to be incorporated within its structure. Yet, it has been a tendency in other fields—particularly the humanities—to criticize as either false or unimportant any notions or methods of representation that run counter to the usages of the subject-predicate grammars which have characterized the development of Western languages. And subject-predicate grammars tend to inject the notion of fixity into our thinking because they are so ill-adapted to express the notions of process and function. Some of our colleagues from the humanities have erred in identifying logic with subject-predicate grammar, and this mistake is one reason for much of their suspicion regarding the rapid increase of training in the sciences where process and function are fundamental. The content of much of the humanities is determined by, if not actually identified with, the structure and usage of traditional language, and in such circumstances it is not difficult to see why cleavages have appeared within ivy-covered walls. Indeed, it is difficult to see how interdepartmental prejudices and the resulting academic disunity can be reduced without due consideration of the linguistic issues involved.

Our thesis so far developed is this: *that a major shortcoming of our general education at the college level has been a failure to recognize linguistic difficulties which effectively prevent many of our students from receiving the kind of unified educational experience to which they are entitled and which they have a right to expect from the liberal arts college.* If the thesis is correct, it indicates one important reason for the failure of interdepartmental and interdivisional understanding. But, on the other hand, it indicates the folly of thinking in terms of a pseudo-integration in which all the areas of intellectual achievement form a perfectly consistent and coherent whole if we would but comprehend it. There is no prospect of such an idyllic Shangri La in education despite the views of some educators. Indeed, difficulties and inconsistencies are the very stimuli needed for intellectual progress, and to remove these

difficulties by fiat is to plunge education into another Dark Age. The history of ideas has been discordant and stormy, and to the extent that his maturity permits the student is entitled to some understanding of the reasons therefor. And finally, the thesis indicates that within the liberal arts college part of this obligation falls on the shoulders of the teacher of physics because of the peculiar nature of his subject.

Ways and Means

It is not unreasonable to suppose that there are at present many teachers of physics in liberal arts colleges who are disposed to examine critically the relation of their subject to the general collegiate enterprise. Of this group there will be some who will agree in principle with the view here set forth, that the physics teacher as educator has certain obligations over and beyond his role as technical physicist. But it is hardly likely that among these last there is any substantial agreement as to the means by which those obligations should be discharged.

Several suggestions have appeared, each reflecting to some extent the personal bias and educational background of the individual making the proposal. At only one point do these proposals agree: they are unanimous in asserting that the enrichment of the physics program shall not be obtained at the expense of that kind of precise thinking and mathematical rigor which has characterized the study of physics in the past. Certainly the elementary course in physics which is diluted by the omission of rigor is in no sense a course in science, no matter what its other virtues may be. If the student fails to come to grips with the mathematical technics and language of his science he has no real notion of the significance of that way of thinking. Knowledge *about* mathematics is no substitute for actual experience in scientific thinking and problem solving. The philosophy of science is important, but its significance is questionable unless predicated upon prior knowledge of at least one exact science, preferably physics. Otherwise the situation is not unlike that of expecting a blind man to appreciate the beauty of a sunset after listening to a verbal description thereof.

But what possibilities are open which will preserve scientific integrity and yet give the student some insight into the relations of his science with the intellectual enterprise as a whole? There is no question that a good course in the philosophy of science taken *after* the student has a preliminary grasp of some scientific area can be of value. But how can we be sure that such a course is a good one? If the course is given by a philosopher whose main interests are in academic philosophy and who has not himself had actual experience with scientific problems, one can be reasonably sure that the outcome will be less than that desired. In all fairness, however, it must be admitted that in recent years there has emerged a small corps of teachers of philosophy whose philosophic interests have developed from training in the sciences. Where such men are available their services should be welcomed.

Perhaps the ideal arrangement is a working agreement between representatives from the sciences and the humanities in which teachers in both divisions cooperate in the common task of investigating interdivisional relationships. But such cooperation is, unfortunately, not easy to obtain. Teachers often fail to recognize as significant those problems that cross interdepartmental lines. Reasons for this situation have already been given.

It is obvious, that in many instances the physicist must take the initiative in showing the importance of his subject in the liberal arts enterprise. If he is so minded he can present the material of elementary physics from the historical point of view, pointing out to the student how current physical concepts have developed from the interaction of new discoveries and new ways of thinking. He can go further and show how the concepts of his science have influenced ways of thinking in other areas. But the success of this procedure rests largely upon the teacher's interest in problems associated with the history, philosophy, language and methodology of physics. Fortunately, teaching materials are available and the method has much to recommend it.

But the lack of standardized procedures should not deter the teacher from attacking the problems here outlined. He can—if he will—aid in the development of teaching technics that will insure the integrity of the liberal arts program. If the same spirit of free inquiry which characterized the development of the *science* of physics is brought to bear on the problems associated with the *teaching* of physics there is ground for optimism concerning the role of physics in those chapters of the history of ideas that still lie ahead.

Purpose as a Factor Affecting the Form and Content of Technical Reports

D. R. WHITE
E. I. du Pont de Nemours & Company, Parlin, New Jersey

TECHNICAL reports form the individual and distinctive part of the literature of the laboratory in which they are prepared. This is true even when they are not published in the technical journals. It is because of the great importance of technical reports that many persons holding positions in industrial laboratories are extremely interested in the steps taken to train students in writing them.

This paper itself has been written as a result of participation in a discussion at the Rochester meeting of the American Association of Physics Teachers, June, 1944. At that time, the writer criticized the college graduate who had majored in physics as being unable to prepare clear reports. This criticism was made as a result of experiences with some college-trained physics majors and more college-trained chemistry majors. This experience indicates that considerable training in report writing is needed before a person with either physics or chemistry background can actually take the degree of responsibility within a commercial organization for which he may be otherwise technically fitted. This appears to be a very real hindrance to progress, at least under the conditions obtaining in our own research organization. Correspondingly, it seems that there is an opportunity for improvement if the problem can be analyzed and effective steps taken to meet it.

Review of the characteristics of a number of reports selected by the leader of our Physics Research Group as rather typical of those with which he has to contend suggested that the trouble was, perhaps, a little more fundamental than poor grammar and inadequate sentence structure. Those were present in some cases, but there appeared an even more basic lack of comprehension of the purpose and functions which can and should be fulfilled by a technical report prepared by a research worker within and for a commercial organization.

The limited purpose—transfer of technical information—is readily recognized, but often not properly evaluated and attacked. Transfer of technical information obviously presupposes a transferor and a transferee, a writer and a reader. If either fails to fulfill his function there will be an incomplete transfer of information. This immediately presupposes a common language carefully and accurately used, so that nice distinctions can be made when necessary and an accurate picture presented where needed. The last part of this paper will deal with general considerations and aids of value in the attainment of this goal—transfer of technical information.

Before continuing with this phase of the matter directly, attention is called to a particular opportunity offered the writer of a technical report. The broad aspects of the work of an industrial laboratory predominantly involve information and judgment in fields far removed from the technical scope of the individual problems presented and solved. Some technically trained men in such an organization may be expected to occupy dual roles requiring dual personalities—one technical, the other, business. The balance often appears to be determined by the personalities and characteristics of the people immediately available. Some scientists wish to and do preserve a rather complete preoccupation with "pure" science, shunning those administrative and business responsibilities attendant upon the operation of a laboratory. Some merge the two, and take both business and technical responsibilities. For advancement in either field, purely technical or a combination with business, technical reports offer a person in a junior position an opportunity to show that he has such grasp of his work that he is ready and able to assume greater responsibilities.

Evidence that this situation is not local in nature is found in a British editorial concerning the infrequency with which chemists were appointed to positions as directors of businesses.[1]

> If anyone thinks it is kindness to a young chemist to train him to write in a particularly didactic style, we are ready to listen to argument. . . . The oftener chemists talk with business men and learn something of their ways of thought the better chance they will have in their later days of talking with works managers and managing directors. On the other hand, they will learn more chemistry if they associate almost exclusively with chemists.

From a broad point of view, a report must convey technical information and also indicate the degree of technical and business competence

[1] Chemistry & Industry **63**, 345 (Sept. 30–Oct. 7, 1943).

of the worker. College programs are rarely so closely integrated that the student is forced to give so many elements careful consideration at one time. A report of a college physics experiment rarely involves problems of business administration or economics, and sometimes is not carefully considered even from the point of view of clarity of language. Yet these and a host of other factors do enter into and affect the judgments formed on the basis of a technical report in an industrial laboratory.

There are other differences between academic and industrial laboratories that affect the form and content of the reports prepared. Most college laboratory work is based upon an "experiment" as a unit. Instructions have been prepared in considerable detail, requisite apparatus is often set out on a table, and the entire program is outlined with the anticipation that the work can be completed in a specified time by that mythical person, "the average student;" and loud are the wails if, perchance, the time schedule is exceeded. The purpose of the experiment is often stated in terms of some quantity that is to be measured; for example, to determine the coefficient of expansion of steel. Actually, of course, this is not the purpose of the experiment at all. Reference to one of the many handbooks containing data in the field would give the coefficient more accurately for the steel specified therein and would often be a better guide than the student's measurements, even for his specific sample. The real purpose of the experiment is pedagogic, to train the student in finger dexterity in making certain types of measurement and to familiarize him with the mensural embodiment of certain laws and relationships stated in the textbook. In an industrial laboratory, even a research laboratory, there are apt to be many routine tests or measurements for which the instructions are worked out in extreme minutia and for which the reporting consists of entering a few numbers in spaces on a prepared form. A laboratory worker can, and should, gain much valuable training and experience from such set experiments, but these alone do not offer complete preparation for the reporting of less standardized tests occurring in a research laboratory. Such non-standardized tests require considerable thought as to method of test and form of presentation of the results. They may involve a measurement of some simple type, or may require a complex series of manipulations; but, in general, the answer is not known, otherwise the test would not be made. Inferential evidence may exist in the hands of some one, leading to the expectation of a certain result, but in the last analysis, the test is being made because the result is qualitatively or quantitatively unknown for the sample under observation. The work and the report thereof must carry that compelling quality which converts the unknown into the known and accepted. This stands in contrast to the requirements of the usual college laboratory report.

Considerable contrast also exists in the origin of the experiments in the college and in the commercial laboratory. The manual of laboratory experiments used in one form or another in so many physics courses has little counterpart in tests of the type primarily considered here. These experiments arise as a means of answering questions from the following sources:

(i) Persons or groups in the research laboratory; these often involve new products or processes.

(ii) Production men in the factory; these often involve problems arising from troubles or emergencies in plant operation.

(iii) Field representatives; these usually involve the use of the product under some definite conditions often peculiar to an individual customer.

These questions may have passed through several steps in following their prescribed course from the point of origin to the laboratory worker finally making the required tests. It is often necessary for someone to review the entire process to be reasonably sure that the right question has been asked or, in any case, the proper report prepared. This need has no counterpart in the normal college laboratory. An instructor would consider it a disgrace to ask the wrong question, but many questions, which must be answered politely by commercial groups, originate with non-technical people who have great faith in science, but who may lack discriminatory knowledge of the possible and impossible, adequate and inadequate, in posing their questions. This leaves much to the judgment and discretion of the technical worker making the tests and writing the report.

Still another difference between the atmosphere of the college and commercial laboratories lies in the difference between the instructor-student and the supervisor-worker relationship. One thing differentiates many of the contacts of the two types. Typically, new information is not being sought in the college laboratory, but it is being sought in the industrial laboratory. In both cases, the person supervising the work may have much broader background and deeper insight

into the fundamental meaning and importance of the experiment than the worker actually making the test. In college the emphasis is all on the understanding and comprehension by the student of material clear to the instructor—the information flow is in that direction. In industry the information flow must be reversed—flowing uphill, if you will,—yet this direction of flow must be maintained from the helper to his superior if the relationship is to have value.

This point has many ramifications. Corresponding to the flow of questions to a research laboratory or research worker, there must be a flow of answers, that is, reports. Most of these reports will have in them an element of this uphill flow of information, the flow from the less informed to the more informed person. This element will be different for different readers of the report. The worker's immediate supervisor will often have broader and deeper technical background, yet information must be conveyed to him. The business heads may be expected to see many economic phases of the matter much more clearly than do any of the technical staff, yet information must also be conveyed to them. A customer who sought technical aid will often see possibilities and limitations of the practical application of suggestions or conclusions far more clearly than does the laboratory worker, yet information must be conveyed to him. The art of successfully conveying information along this uphill path deserves careful thought. It is not the normal trend in colleges, and when a student tries to correct or instruct an instructor, the results are not always happy. Conclusions or suggestions so phrased that they are interpreted as personal criticism fare no better in a commercial laboratory.

The last difference to which attention is called is the usual difference in responsibility for action between the academic instructor and the business executive. Rarely is any specific action to be taken as a result of college laboratory experiments. The instructor may classify the work as good or poor, accurate or inaccurate, neat or sloppy, and may grade it accordingly, but the poorly done experiment has not been any more expensive than the well done. No one has been misled nor any money wasted directly as a result of the poor work. On the other hand, there is often a close connection between dollars and cents and industrial technical reports, even those that are quite elementary. Incorrect data or faulty conclusions may lead directly to losses. It is not surprising that the executives who bear the ultimate responsibility should test the accuracy and precision of thinking of all their subordinates by all the means at their command. Such a process is not infallible, and sometimes leads to strange bases of judgment, but is quite understandable.

These remarks have outlined several points bearing on technical-report writing and college preparation therefor; the next paragraphs will present some suggestions concerning specific points that should receive attention in writing reports.

Reports should conform to local practices of presentation. The strict routine report has been excluded from this consideration, but general forms and arrangements are often prescribed even when considerable latitude must be left for the presentation of detailed data. In our own laboratory, these arrangements include presentation of a summary and conclusions very early in the report, typically on the first page of any report that is more than one page in length, and often as a brief titled paragraph at the top of the sheet in a single-page report. Although this order is not the logical order of a carefully planned argument, it does fulfil a very definite need by saving the time of busy people. It is successful in this respect only when the reader can place confidence in the competence of the workers presenting the reports.

The report as a whole, and particularly the summary, must be carefully worded in order to present briefly the scope of the work and the conclusions reached. The importance of this factor is widely recognized, and will not be further emphasized here.

Beyond this one item of summary and conclusion little else is fixed. The reports vary in size, method of presentation of data, purpose, and readers anticipated. Some have obvious and distinct legal importance, most frequently, perhaps, in connection with patent actions. In such cases, beyond the classes of readers already mentioned, is the lawyer, with his rules and practices requiring special attention which often seems onerous and burdensome. Probably one reason for this feeling is the fact that academic training in science usually leaves out this phase of endeavor, fundamental though it is to our present economic system. The legal requirements of reports of patent interest often supersede the other reporting practices and require special forms or certain duplication in reporting. Early familiarity

should be acquired with this aspect of the work of a commercial laboratory.

At times the real and final conclusions depend upon evaluation of many factors, upon some of which the laboratory worker does not feel competent to pass judgment. In this case, particular care is needed to be sure that adequate technical data are presented to furnish a suitable basis for the conclusion which someone must draw from the work. The diverse backgrounds and requirements of the different readers who may have occasion to read the same report has been emphasized. The business man must make a decision upon the basis of the work. The results must be so clear to him that he can proceed correctly and with confidence to take the next steps, advantageous for the business. The director of research must find in the report evidence of work adequately done. The physicist immediately responsible for the work must find evidence of technical competence in his own special field. All this must be within the confines of one brief report.

People differ in their estimate of desirable forms of presentation of data, and unexpected criticisms of reports are made at times. The writer knows of one instance where a report was read by the head of an organization, and a scathing blast came back that the report was very bad—the tables of data were presented throughout the text instead of being placed at the end. No comment was ever made, to the writer's knowledge, on the way in which the job had been done, but the report *per se* was unsatisfactory. As another illustration of lengths to which some things are carried, the writer has known of an attempt to put certain technical matters in a form that would prove acceptable to several other business concerns. One of the obstacles encountered was the reluctance of a large company, noted for its scientific achievements and support of research, to see any ideas expressed either as integrals or summations in such writing. This was true even though that form of expression was the most precise which could be found to express exactly what was meant, and is familiar to people of the degree of technical training whose interest was anticipated.

Each report prepared in a research laboratory has a relationship to some other work done there or to some special phase or aspect of the business of the organization. These relationships should be shown briefly but adequately in any report. In simple cases this may require reference only to a request or work order which authorized the work. In other cases, more detail may be required to properly orient this specific bit of work to its surroundings. The urge for brevity, continually present in such writing, is sometimes misconstrued as justification for omission of these details. The damage done by such omission may not be apparent at the instant, but becomes evident when reference is made to the work at a later date. It often happens that these elements of relationship to other work are vital to an adequate understanding of the report, and though the readers may happen to remember these details when the report is fresh, they may be completely forgotten months later when reference is made to it.

The detail used in the presentation of technical aspects of the work varies from report to report. Two considerations determine fundamentally the extent to which these details are given. The report should include enough of the details of the work to show its technical adequacy. This is primarily intended for reading and understanding by technically trained people, but should be worded in such a way that any intelligent reader will feel that he has read a well-planned piece of work even if he is not familiar with the specific technical field. Again, the report should give enough of the details of the work to permit re-check or extension at a later date. Here, as in the case of the relationship of the work to other work, the urge for brevity is sometimes used as the excuse for omissions. Such omissions are serious and should be scrupulously avoided.

It appears to be a common practice for management to view technical reports with a certain amount of suspicion, or perhaps better, a certain amount of skepticism. Possibly that is the way general managers "get that way." Each step leading to conclusions involving the basic economics of the conduct of a business should be tested and tried lest the entire structure erected be unsound. Errors are still left after the best scrutiny. The writer of a technical report should never demand blind trust in his work, but he should earn the confidence of his associates and superiors and each new report should be designed to present its own bit of evidence of competence.

These comments have dealt with the purposes that can be accomplished by a properly prepared technical report. As a minimum, it transmits technical information, and beyond that often is taken to show the technical and business compe-

tence of its author. Some important differences between college and industrial laboratories have been pointed out, as they affect the purpose, form and content of reports of experimental work. A brief outline has been given of practices that have been adopted in one laboratory as an aid in securing adequate reports. It is hoped that these remarks may be of some value to those formulating courses and policies in the teaching of physics.

Wave Velocities in Elementary Physics

S. MILLMAN
Queens College, Flushing, New York
AND
M. W. ZEMANSKY
The City College, New York, New York

IT is generally regarded as good pedagogy to provide a student of elementary physics with a derivation of an important equation, even though the derivation may lack rigor or great generality. Thus, the acceleration of a particle moving with constant speed in a circle is usually derived in textbooks of college physics in an elementary way that avoids the use of the calculus. That this derivation is better than none at all is freely conceded, particularly in view of the fact that it gives the student further understanding of the vector character of velocity, the method of subtraction of vectors, the meaning of uniform motion, and so on—ideas that deserve constant reiteration. It follows that a non-rigorous derivation of an equation in elementary physics serves two purposes: first, to give the student a rough idea as to how the equation comes about; second, to give him further practice in using previously learned definitions and principles.

It is unfortunate, therefore, that so little attention has been paid to simple non-rigorous derivations of the expressions for the speeds of the fundamental types of waves whose other properties are studied somewhat extensively in elementary physics. Such derivations not only make clear why the wave speed depends only on particular properties of the medium, but also provide a further justification for the introduction of the various moduli which the student encountered in his study of elasticity. The volume, or bulk, modulus of a substance will mean more to a beginner when he can see how it enters into the expression for the speed of a longitudinal wave; and similarly, the "spring constant" will have more meaning when the student can see how it (among other properties) determines the speed of a longitudinal wave in a spring.

It is interesting that the one derivation of wave speed given in most textbooks of college physics, namely, that of the speed of a transverse wave in a string, involves the rather sophisticated procedure of imagining the string itself to move with a speed v equal to the wave speed but in the opposite direction. The centripetal acceleration of a particle of the string becomes v^2/R, with the result that many students are left with the impression that the particle speed and the wave speed are identical—a bit of confusion that should certainly be avoided.

It is our purpose to point out that simple, although not general, derivations of the expressions for the speed of *all* types of waves of interest in a first course can be given without the use of calculus. Application is made of two of the most important relations of elementary physics, namely, $\sum F = ma$, and $s = \frac{1}{2}at^2$; and at no time is it necessary to imagine the medium moving backward with a speed equal to that of the wave

Outline of the Method

(*a*) Consider a convenient portion of the medium, and imagine a very small displacement

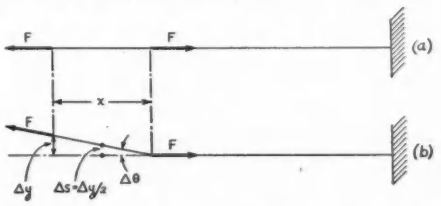


FIG. 1. Propagation of a transverse disturbance in a string

(longitudinal Δx or transverse Δy) at one end, the displacement constituting the very beginning of an oscillation.

(*b*) Allow the resulting disturbance to travel in one direction along the medium a distance x with a constant speed v in a time $t=x/v$.

(*c*) Calculate the mass m of material between the planes at $x=0$ and x which has been moved as a result of the propagation of the disturbance.

(*d*) Calculate the displacement Δs of the center of mass of this material in terms of the magnitude of the original displacement.

(*e*) Assume the motion of the center of mass to be one of constant acceleration, so that the acceleration a is given by $\Delta s=\frac{1}{2}at^2$, or $a=2\Delta s/(x/v)^2$.

(*f*) Calculate the unbalanced force $\sum F$ acting on this mass, assuming it to be constant during the time interval t.

(*g*) Apply Newton's second law, $\sum F=ma$, and solve for v.

The method is, of course, rough and may be applied only to waves which travel with a speed independent of the wave form, that is, to waves in nondispersive mediums. In the following section, this method is applied to five elementary types of wave.

Applications

1. *Transverse wave in a stretched string.*—In Fig. 1(*a*) is shown an undisturbed string of linear density σ, and in Fig. 1(*b*) the state of the string after a short time interval t during which the end of the string has been displaced transversally by a small amount Δy and the disturbance has traveled along the string a distance x. The string to the right of this point is in its undisturbed state under a force F. To a first approximation we may consider the shape of the disturbed portion of the string to be linear, so that its center of mass will have been displaced upward an amount $\Delta s=\frac{1}{2}\Delta y$. Assuming the unbalanced force acting on this portion of the string of length x to be constant, we have

$$\sum F_y=F\sin\Delta\theta=F\Delta\theta=F\Delta y/x,$$

$$m=\sigma x,$$

$$a=\frac{2\Delta s}{t^2}=\frac{\Delta y}{(x/v)^2}.$$

Hence,

$$F\frac{\Delta y}{x}=\sigma x\cdot\frac{\Delta y}{(x/v)^2},$$

or

$$v=(F/\sigma)^{\frac{1}{2}}.$$

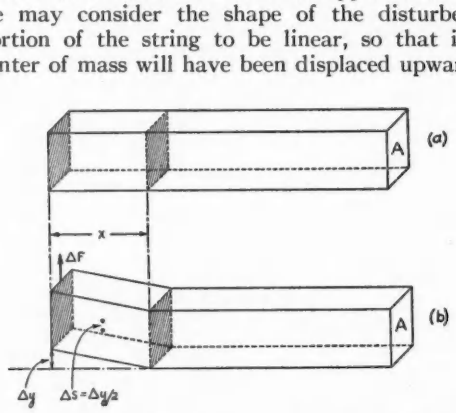


FIG. 2. Propagation of a transverse disturbance in a solid.

2. *Transverse wave in a solid.*—Fig. 2(*a*) shows an undisturbed medium, of cross-sectional area A and density ρ, and Fig. 2(*b*) shows a transverse displacement Δy of one end brought about by the action of a shearing force ΔF. We have

$$\sum F_y=\Delta F,$$

$$m=\rho Ax,$$

$$a=\frac{2\Delta s}{t^2}=\frac{\Delta y}{(x/v)^2}.$$

Hence

$$\Delta F=\rho Ax\cdot\frac{\Delta y}{(x/v)^2},$$

or

$$v=\left(\frac{\Delta F/A}{\Delta y/x}\Big/\rho\right)^{\frac{1}{2}}=\left(\frac{S}{\rho}\right)^{\frac{1}{2}},$$

where S is the shear modulus.

3. *Longitudinal wave in a stretched spring.*—Fig. 3(*a*) shows an unstretched spring of length L_0 and linear density σ_0; Fig. 3(*b*), the spring stretched to a length L by a force F; and Fig. 3(*c*), a displacement Δx given to the left-hand end, causing a disturbance to travel a distance x. We

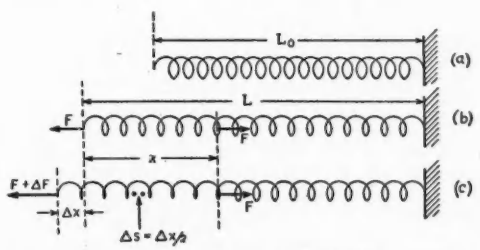


FIG. 3. Propagation of a longitudinal disturbance in a stretched spring.

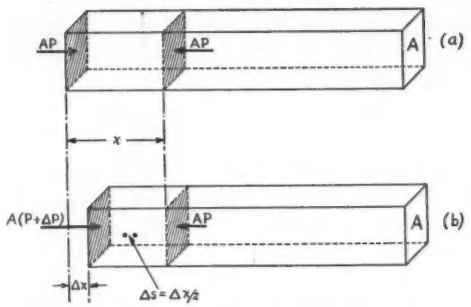


FIG. 4. Propagation of a longitudinal disturbance in a solid or fluid.

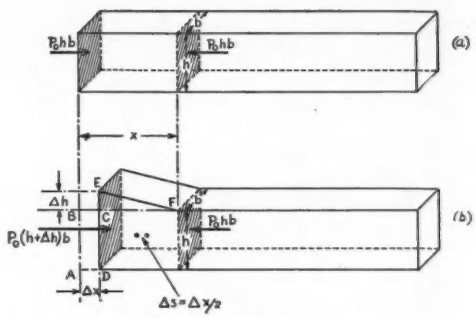


FIG. 5. Propagation of a disturbance in water in a shallow channel.

have

$$\sum F_x = \Delta F,$$

$$m = (L_0/L)\sigma_0 x,$$

$$a = 2\Delta s/t^2 = \Delta x/(x/v)^2.$$

Hence

$$\Delta F = (L_0/L)\sigma_0 x \cdot \Delta x/(x/v)^2,$$

and

$$v = \left(\frac{\Delta F}{\Delta x/x} \Big/ \sigma_0 \cdot \frac{L}{L_0}\right)^{\frac{1}{2}}.$$

Now

$$\frac{\Delta F}{\Delta x/x} = \frac{F}{(L-L_0)/L_0} = kL_0,$$

where $k[=F/(L-L_0)]$ is the "spring constant." Hence

$$v = (kL/\sigma_0)^{\frac{1}{2}}.$$

4. *Longitudinal wave in a solid or fluid.*—In Fig. 4(*a*) is shown the undisturbed medium; and in Fig. 4(*b*), a small displacement Δx of one end, the disturbance having traveled a distance x. We have

$$\sum F_x = A\Delta P,$$

$$m = \rho A x,$$

$$a = 2\Delta s/t^2 = \Delta x/(x/v)^2.$$

Hence

$$A\Delta P = \rho A x \cdot \Delta x/(x/v)^2,$$

or

$$v = \left(\frac{\Delta P}{A\Delta x/Ax} \Big/ \rho\right)^{\frac{1}{2}} = \left(\frac{\Delta P}{\Delta V/V} \Big/ \rho\right)^{\frac{1}{2}} = \left(\frac{B}{\rho}\right)^{\frac{1}{2}},$$

where V is the volume and B is the volume modulus. This result may be written also as follows:

$$v = \left(\frac{\Delta F/A}{\Delta x/x} \Big/ \rho\right)^{\frac{1}{2}} = \left(\frac{Y}{\rho}\right)^{\frac{1}{2}},$$

where Y is Young's modulus, since, under these simplified conditions, the volume modulus and Young's modulus are identical.

5. *Long wave in shallow water.*—A type of water wave that can be handled by elementary methods is the one whose speed is independent of the wavelength. This is the wave which is propagated in a shallow channel in such a manner that all the particles in a vertical plane move together, the vertical motion being negligibly small. The horizontal motion is assumed to be such that, by Bernoulli's principle, the average pressure P_0 is independent of the elevation and equal to that at an undisturbed place where the depth is $\frac{1}{2}h$. The undisturbed medium is shown in Fig. 5(*a*), and a longitudinal displacement Δx, causing a disturbance to travel a distance x, is shown in Fig. 5(*b*). The elevation Δh of the left-hand end is calculated in terms of Δx by equating the area of the rectangle $ABCD$ to the area of the triangle CEF. Thus

$$h\Delta x = \tfrac{1}{2}\Delta h \cdot x,$$

whence

$$\Delta h = 2h\Delta x/x.$$

We have

$$\sum F_x = P_0[(h+\Delta h)b - hb]$$

$$= \tfrac{1}{2}h\rho g \cdot b\Delta h$$

$$= \tfrac{1}{2}h\rho gb \cdot 2h\Delta x/x$$

$$= h^2\rho gb\Delta x/x,$$

$$m = \rho hxb,$$

$$a = 2\Delta s/t^2 = \Delta x/(x/v)^2.$$

Hence

$$h^2\rho gb\Delta x/x = \rho hxb \cdot \Delta x/(x/v)^2,$$

or

$$v = (gh)^{\frac{1}{2}}.$$

The Actual Path of a Projectile in a Vacuum

J. B. SCARBOROUGH
U. S. Naval Academy, Annapolis, Maryland

THE motion of a projectile in a vacuum is discussed in the majority of undergraduate textbooks on analytic geometry, calculus and mechanics. In deriving the equation of the path, the authors start with a set of fixed rectangular coordinate axes with origin at the muzzle of the gun, y-axis vertical and x-axis horizontal. The acceleration owing to gravity is assumed to be (i) constant in magnitude and (ii) parallel to the y-axis for every point in the path. Neither of these assumptions is strictly correct, as the acceleration decreases with height above the earth's surface and is always directed toward the center of the earth. As a consequence of these incorrect assumptions, the path of the projectile always turns out to be a parabola. In reality, however, if a projectile were fired from a fixed earth and with any velocity practically attainable, its path in a vacuum would be an ellipse. This fact has been known for a long time, but textbook writers seem to have overlooked it.

The objects of this paper are (*a*) to derive in a direct and elementary manner the equation of the actual path of a projectile in a vacuum and (*b*) to compare by means of a numerical example the approximate parabolic path with the true elliptic path in the case of a projectile of moderately high initial velocity.

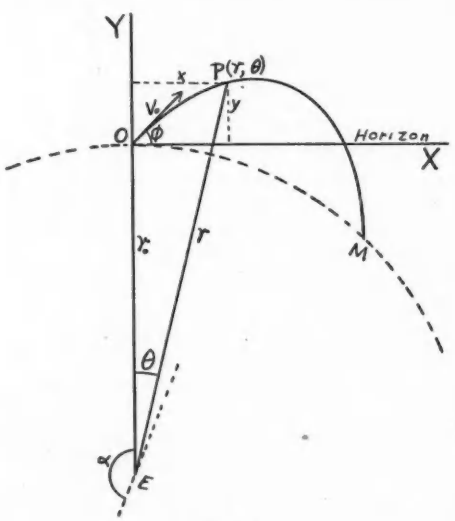


FIG. 1.

Equation of the Path

In Fig. 1 the dashed circle represents a section of the earth's surface made by a plane passing through the center E of the earth and the point O. Assume the earth at rest* (not rotating) and suppose a material particle projected from O with initial velocity v_0 in the plane EOM and at an angle φ with the horizon at O. Let r_0 denote the radius of the earth, g_0 the acceleration due to gravity at O, and $P(r, \theta)$ any point in the path of the particle.

The acceleration g at $P(r, \theta)$ is equal to $g_0 r_0^2/r^2$ and is directed toward E along r. The tangential component of g at $P(r, \theta)$ is $-g \cos \psi$ (see Fig. 2). From the relations $\tan \psi = r\mathrm{d}\theta/\mathrm{d}r$ and $\mathrm{d}s = [(\mathrm{d}r)^2 + (r\mathrm{d}\theta)^2]^{\frac{1}{2}}$, $\cos \psi = \mathrm{d}r/\mathrm{d}s$. Then the tangential acceleration at $P(r, \theta)$ is

$$\frac{\mathrm{d}v}{\mathrm{d}t} = -g\frac{\mathrm{d}r}{\mathrm{d}s} = -g_0\frac{r_0^2\,\mathrm{d}r}{r^2\,\mathrm{d}s} = -\frac{k\,\mathrm{d}r}{r^2\,\mathrm{d}s};$$

whence

$$\mathrm{d}v = -\frac{k}{r^2}\frac{\mathrm{d}t}{\mathrm{d}s}\mathrm{d}r = \frac{-k\mathrm{d}r}{r^2 v},$$

or $v\mathrm{d}v = -k\mathrm{d}r/r^2$, where k equals $g_0 r_0^2$. Integrating

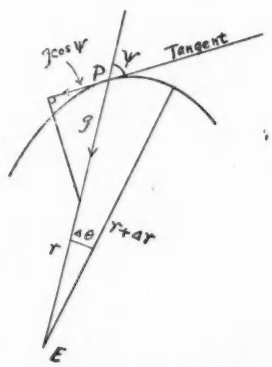


FIG. 2.

* This assumption is necessary in order that the path of the projectile be a plane curve. The rotation of the earth causes a projectile to swerve slightly from the original plane of fire—to the right in the northern hemisphere and to the left in the southern hemisphere.

the last equation between v_0 and v, and r_0 and r, we get

$$v^2=\frac{2k}{r}+q, \tag{1}$$

where $q=v_0^2-(2k/r_0)$.

Since the acceleration of the particle is always toward E, the transverse acceleration

$$a_\theta=\frac{1}{r}\frac{\mathrm{d}}{\mathrm{d}t}\left(r^2\frac{\mathrm{d}\theta}{\mathrm{d}t}\right)$$

is zero. Hence $r^2\mathrm{d}\theta/\mathrm{d}t=C$, a constant. Since $r_0\mathrm{d}\theta/\mathrm{d}t=v_0\cos\varphi$ at the point O,

$$C=r_0\cdot r_0(\mathrm{d}\theta/\mathrm{d}t)_0=r_0v_0\cos\varphi. \tag{2}$$

The velocity along a polar curve is given by

$$v^2=\left(\frac{\mathrm{d}r}{\mathrm{d}t}\right)^2+r^2\left(\frac{\mathrm{d}\theta}{\mathrm{d}t}\right)^2.$$

Since $\mathrm{d}\theta/\mathrm{d}t=C/r^2$ and

$$\frac{\mathrm{d}r}{\mathrm{d}t}=\frac{\mathrm{d}r}{\mathrm{d}\theta}\cdot\frac{\mathrm{d}\theta}{\mathrm{d}t}=\frac{C}{r^2}\frac{\mathrm{d}r}{\mathrm{d}\theta}, \quad v^2=\frac{C^2}{r^4}\left(\frac{\mathrm{d}r}{\mathrm{d}\theta}\right)^2+\frac{C^2}{r^2}.$$

Comparing with Eq. (1) and solving for $\mathrm{d}\theta$, we obtain

$$\mathrm{d}\theta=\frac{C\mathrm{d}r}{r(qr^2+2kr-C^2)^{\frac{1}{2}}}.$$

Integrating between 0 and θ, r_0 and r, and simplifying the result, we get

$$r=\frac{C^2/k}{1+[1+q(C/k)^2]^{\frac{1}{2}}\cos(\theta+\alpha)}, \tag{3}$$

where

$$\alpha=\cos^{-1}\left[\frac{(C^2/r_0)-k}{(k^2+qC^2)^{\frac{1}{2}}}\right].$$

This is the equation of a conic in which the eccentricity e is $(1+q(C/k)^2)^{\frac{1}{2}}$. When q, C and k are replaced by their previously determined values, the eccentricity becomes

$$e=\left[1+\frac{v_0^2\cos^2\varphi(v_0^2-2g_0r_0)}{g_0^2r_0^2}\right]^{\frac{1}{2}}.$$

The path will be a parabola only when $e=1$, or $v_0^2=2g_0r_0$. For $r_0=6{,}370{,}300$ m (about 3958.4 mi) and $g_0=9.81$ m/sec^2, we get $v_0=11{,}180$ m/sec $=6.95$ mi/sec. For any initial velocity less than this the path will be an ellipse.

Numerical example.—Taking $v_0=820$ m/sec (2690 ft/sec), $\varphi=44°$, $g_0=9.81$ m/sec^2 and $r_0=6{,}370{,}300$ m, we find

$$\alpha=179°\ 41'\ 24''\ .80758920,$$
$$e=[1+q(C/k)^2]^{\frac{1}{2}}=0.994446947749324,$$
$$C^2/k=35467.1978380612 \text{ m}.$$

The polar equation of the path is then, from Eq. (3),

$$r=\frac{35467.1978380612}{1+0.994446947749324\cos(\theta+\alpha)}. \tag{4}$$

As a check on this equation, we find that for $\theta=0$, $r=6{,}370{,}299.999{,}999$, which differs from r_0 by one unit in the 13th figure, or by less than 1/25,000 in.

The range of this projectile, measured on the surface of the earth, is found from the familiar formula $s=r\theta$, where the central angle is $2(180°-\alpha)=0°\ 37'\ 10''\ .3848216$ $=0.0108132107$ radian. Hence, the range is 6,370,300 $\times 0.0108132107=68{,}883.396$ m, or about 42.8 mi.

The Elliptic and Parabolic Paths Compared

In order to compare the approximate parabolic path of a projectile with the true elliptic path for the same data, it is necessary that the coordinates of both curves be referred to the same set of axes. The curve of Fig. 1 represents the elliptic path. With reference to the axes there shown, the rectangular coordinates of points on the ellipse are given by the relations

$$x=r\sin\theta, \quad y=r\cos\theta-r_0. \tag{5}$$

The equation of the parabolic path is

$$y=x\tan\varphi-\frac{g_0x^2}{2v_0^2\cos^2\varphi}.$$

For $\varphi=44°$, $g_0=9.81$ m/sec^2, $v_0=820$ m/sec, this becomes

$$y=x(0.965688774807074 -0.000014097533227263 6x). \tag{6}$$

Since both curves are referred to the same axes, the difference between the ordinates y_e and y_p for the same abscissa will show the amount of disagreement in the two paths at that abscissa. The lack of agreement is given in the last column, headed y_e-y_p, in Table I. The values of r, y_e and y_p were computed from Eqs. (4), (5) and (6), respectively.

Inspection of the last column shows that the paths differ appreciably by the time the projectile has covered one fourth of its range, and that the difference is quite marked after the maximum ordinate is reached. The inherent error in the parabolic path is not large when expressed as a percentage (the maximum error being about 1 percent of the ordinate concerned), but its absolute value is large enough to cause a gunner to

miss a good-sized skyscraper by shooting over it if he aimed at the center of its side and assumed a parabolic path for the projectile.

Remarks

Although this paper is concerned mostly with academic matters, since it deals with the motion of a projectile in a vacuum, the results have some bearing on real trajectories in air. If the entire trajectory in air be computed with reference to a single set of axes fixed at the muzzle of the gun, and the acceleration owing to gravity be assumed constant in magnitude and direction throughout the path, the computed path will be erroneous to some extent because of the incorrect assumptions with respect to g; but the errors will be smaller than those for the vacuum path because of the lesser magnitude of the trajectory for the same initial conditions. If the path be computed by the process of numerical integration, however, these errors can be prevented, as that process automatically takes into account the changing direction of g and also permits the computer to use the proper value of g at any height.

TABLE I. Comparison of the elliptic and parabolic paths. All distances are in meters.

θ	r	x $(=r\sin\theta)$	y_e $(=r\cos\theta-r_0)$	y_p	y_e-y_p
0′ 00″	6370300.0000	0.0000	0.000	0.000	0.000
2′ 30″	6374475.5329	4635.6490	4173.847	4173.649	0.198
5′ 00″	6378053.2901	9276.4992	7746.544	7745.070	1.474
7′ 30″	6381031.2472	13921.2396	10716.061	10711.470	4.591
10′ 00″	6383407.7165	18568.5542	13080.710	13070.739	9.971
12′ 30″	6385181.3495	23217.1234	14839.140	14821.454	17.686
15′ 00″	6386351.1389	27865.6252	15990.345	15962.885	27.460
17′ 30″	6386916.4196	32512.7364	16533.666	16494.992	38.674
20′ 00″	6386876.8704	37157.1338	16468.784	16418.431	50.353
22′ 30″	6386232.5137	41797.4956	15795.731	15734.550	61.181
25′ 00″	6384983.7159	46432.5026	14514.882	14445.385	69.497
27′ 30″	6383131.1867	51060.8393	12626.957	12553.659	73.298
30′ 00″	6380675.9782	55681.1954	10133.022	10062.776	70.246
32′ 30″	6377619.4834	60292.2669	7034.484	6976.812	57.672
35′ 00″	6373963.4341	64892.7571	3333.092	3300.509	33.583
37′ 10″ .3848216	6370300.0000	68882.0541	−372.422	−370.466	−1.956

Imaging of Underwater Objects

LAWRENCE E. KINSLER*
United States Naval Academy, Annapolis, Maryland

IT is a well-known phenomenon that the apparent position of an underwater object when viewed from above the surface does not coincide with its true position. The correct relationship between these two positions is of particular importance for certain aspects of naval warfare such as the visual observation of unknown reefs in uncharted waters or the observation and attack of submerged submarines from the air. Although the relationship between these two positions has been discussed in brief notes in this journal,[1] there still exists much confusion and misinformation on the subject. Cases could be cited of textbooks and technical publications of recent date that are either in error or in need of clarification in their treatment of the phenomenon. It is the purpose of this paper to offer both experimental and theoretical proof of the correct relationship and to propose an explanation suitable for use in elementary textbooks.

In general, when a bundle of rays proceeding from a point source of light in water is incident at the water surface, it is refracted into a system of rays that is not homocentric. The only exception to this statement is the simple case of a bundle of paraxial rays striking the surface normally. Then the new center of the refracted rays is above the source by such an amount that the ratio of the true depth to the apparent depth equals the refractive index of the water. This explains the commonly experienced observation that upon looking directly down into water an undistorted image is seen at an apparent depth only 1/1.33, or about 75 percent, of the true depth. However, in the general case, when the rays from the underwater source strike the surface at a considerable angle, 10° or more, an astigmatic bundle of refracted rays is obtained.

From the curvature of the refracted wave surface it is possible to show that the caustic curve containing all images produced by the intersection of rays in a vertical plane is the curve $MS'V$ in Fig. 1. If this curve is rotated about the y axis, a caustic surface containing all possible *horizontal* astigmatic images is obtained. Similarly, considering rays in a plane at right angles to the vertical plane, they are all found to intersect on the line AM. Thus the caustic surface containing all *vertical* astigmatic images degenerates in this case into the line segment AM. By

* Lieutenant Commander, USNR. The assertions contained herein are the private ones of the writer and are not to be construed as official or reflecting the views of the Navy Department or of the naval service at large.

[1] E. R. Laird, Am. J. Phys. (Am. Phys. T.) 6, 40 (1938); H. M. Reese, *ibid.* 6, 163 (1938); G. Arvidsson, *ibid.* 6, 164 (1938).

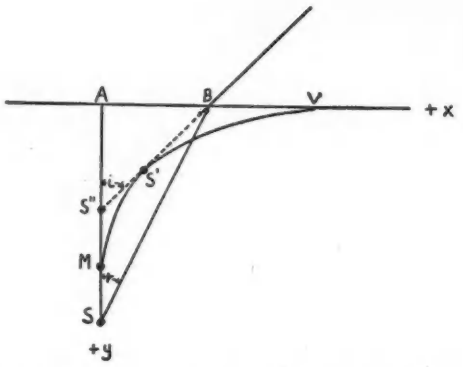


FIG. 1. Caustic curves by refraction at a water surface.

means of simple trigonometry one may show that the position y'' of the vertical astigmatic image is given by the equation

$$y''=\frac{d(1-n^2\sin^2 r)^{\frac{1}{2}}}{n\cos r}, \tag{1}$$

where n is the refractive index of water, and $d[=AS]$ is the depth of the source. Considering only rays in the xy plane, one may show that the parametric equations of the caustic curve are

$$x'=d(n^2-1)\tan^3 r, \tag{2}$$

$$y'=(d/n)[1-(n^2-1)\tan^2 r]^{\frac{3}{2}}. \tag{3}$$

These two equations also give the coordinates of the horizontal astigmatic image. From these equations it is possible to show that the astigmatic difference between any two corresponding images S', S'' is

$$S'S''=\frac{d(n^2-1)\tan^2 r}{n\cos r}. \tag{4}$$

It is possible to check the validity of these equations by experimentation with a microscope of long focal length, an optical flat, and a sheet of cross-ruled paper. The microscope is adjusted so that its line of sight makes some definite angle with the normal to the optical flat. The cross-ruled paper is placed under the optical flat. The astigmatic difference $S'S''$ is then the distance that the microscope tube must be moved from the position where the vertical lines are in sharp focus to that in which the horizontal lines are in sharp focus. Table I shows calculated and observed values for various viewing angles of observation i, where $i=\sin^{-1}(n\sin r)$. Replacement of the microscope with a simple magnifying glass makes it possible to give an excellent qualitative demonstration that the emerging wave front is astigmatic.

TABLE I. Experiment with slab of glass of thickness d, 2.00 cm; refractive index n, 1.51.

Viewing angle i (deg)	Astigmatic difference $S'S''$ (cm)	
	Obs.	Calc. [Eq. (4)]
70	1.42	1.38
60	1.06	1.03
50	0.70	0.70
40	.40	.41
30	.21	.22
20	.11	.10
10	.02	.025

Returning to the observation of underwater objects, it is apparent that if our perception of distance depended upon observation with only one eye, there would be no single solution for the relationship between true and apparent position. The apparent position could be anywhere between the two astigmatic images. However, binocular vision results in at least two definite, unique positions. If the normal method of observation with the head erect and the eyes in a horizontal line is used, the curvature of the wave front is such that the two bundles of rays entering the eyes appear to come from the position of the *vertical* astigmatic image S''. But if the head is tilted so that the eyes lie in a vertical line, the curvature of the wave front is different and is such that the two bundles of rays now appear to come from the position of the *horizontal* astigmatic image S'. The validity of these statements may be checked by observing, at a large angle of incidence, an object immersed in water in a shallow vessel, first with the head erect and then with it tilted through 90°. In the latter case the object will appear both nearer to the observer and closer to the surface.

Since it is customary to observe underwater objects with both eyes and with the head erect,

TABLE II. Positions of the two images of an object at a depth d of 100 ft; $n=1.33$.

Viewing angle i (deg)	Apparent depth		Horizontal displacement x' (ft)
	y'' (ft)	y' (ft)	
0	75.2	75.2	0
10	75.0	73.6	0.2
20	73.1	69.0	1.5
30	70.4	61.3	5.2
40	65.8	50.3	12.0
50	59.1	36.4	27.0
60	49.5	21.6	48.4
70	36.2	8.6	76.0
80	19.4	1.2	103.0
90	0	0	115.0

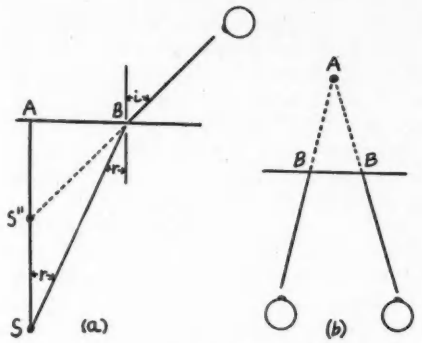


FIG. 2. Apparent position of underwater object.

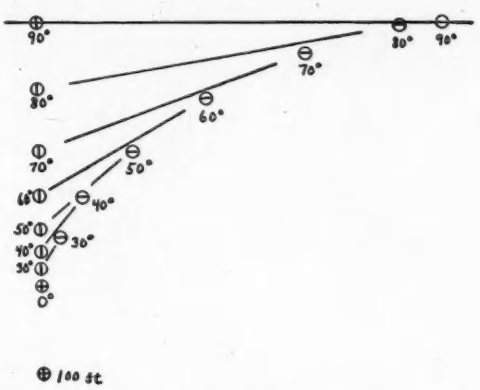


FIG. 3. Shift of apparent position with viewing method and viewing angle.

an elementary treatment of the phenomenon may be limited to this solution alone. Figure 2 suggests a possible method: Fig. 2 (*a*) shows that refraction at the surface determines the apparent direction of the object; Fig. 2 (*b*) shows that since the plane of the incident ray and the normal is the same as that of the refracted ray and normal, the planes of the two incident rays in the water and of the two refracted rays in the air will intersect in the same line AS. Therefore, the usual apparent position of an underwater object is determined by the intersection of the line of apparent direction BS'' as observed in air with the vertical line AS above the object.

Table II gives the positions of the two images in water for various viewing angles. The object is assumed to be at a true depth of 100 ft. Figure 3 presents the same information in graphical form. In this figure the two apparent positions for any one viewing angle are labeled with the numerical value of the viewing angle in air. From these data it is quite apparent that a serious error is made if the incorrect apparent position is used in drawing conclusions as to the true location of underwater objects.

A Problem and an Experiment on Horizontal Acceleration

RICHARD M. SUTTON
Haverford College, Haverford, Pennsylvania

I. The Problem

A COMMON way of accelerating a car of mass M_1 horizontally is shown in Fig. 1, Case *A*. Two less conventional ways are shown in Cases *B* and *C*. Consider M_1 and M_2 to be the same in each of the three cases, but not necessarily equal to each other. Neglect friction and rotational inertia of wheels and pulleys; and, in Cases *B* and *C*, neglect the weight of the movable pulley. Then three questions arise:

(1) In which case will the acceleration of M_1 be greatest?

(2) Is any one of the systems capable of imparting to M_1 an acceleration equal to, or greater than g, the acceleration due to gravity?

(3) If one or more of the systems satisfies the condition of Question 2, what should be the ratio M_2/M_1 for the system if the horizontal acceleration of M_1 is to be equal to g?

The reader with sporting blood in his veins may want to stop at this point and answer the foregoing questions before reading farther. He is advised to watch his step!

II. The Experiment

The following experiment is offered as an interesting and useful addition to the lecture table and laboratory repertory. It occurred to the writer that, accustomed though we are to seeing bodies fall freely under gravity, we are not so accustomed to observing for measurable intervals of time such high values of acceleration in other than the vertical direction. How could one produce a steady and predictable acceleration of

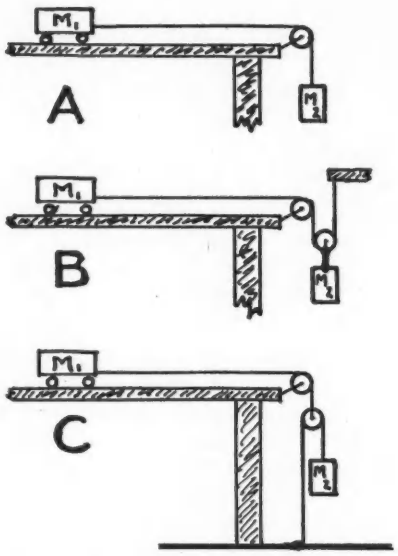


FIG. 1. Three ways of accelerating the car.

32.2 ft/sec² in a car or other object moving horizontally? How would one then demonstrate that the object actually had an acceleration as great as that of free fall?

The arrangement shown in Fig. 2 was found to fulfil the requirements. To the reader it may appear at once as a gross perversion of Atwood's machine with "a mechanical disadvantage of 2." A small laboratory car equipped with a box has a total mass M_1. A cord connected to the car passes over a fixed pulley F at the end of the lecture table, thence around a movable pulley P to a fixed point Y. From the movable pulley is hung a mass M_2 equal to $4M_1$. Then the weight of M_2 is sufficient to give the car the acceleration g when the system is released. When the system is at rest, the force in each of the supporting cords to the movable pulley P is $2M_1g$, or just twice the force required to give M_1 an acceleration g. However, as soon as the car is released and the system is allowed to accelerate freely, the movable pulley and the mass M_2 descend with acceleration $\frac{1}{2}g$, and the force in each of the supporting cords is therefore only M_1g, or half of the value that existed before the system was released.

To demonstrate that the acceleration of M_1 is actually equal to that of free fall, a steel ball is dropped from B (Fig. 2) at the instant that the car is released at A. The distance BC is made equal to the distance AC at the beginning of the run. If all adjustments are well made, *the ball drops into the box* as the accelerated car passes the point C! The length of the box on the car obviously provides some slight margin for experimental errors; but errors of timing of the order of 0.02 sec are sufficient to make the experiment fail, even when d is 1 m.

A ball that does not bounce (made of putty, for instance) may save the operator the embarrassment of apparent failures caused by the escape of the ball from the box after a fair catch. The manner of release need not be complicated. Ball and car might, of course, be released simultaneously by breaking the circuits of two retaining electromagnets connected in series. However, it is much simpler to suspend the ball at the point B by a thread passing over a horizontal rod R. The operator at O then grasps between his thumb and fingers the thread holding the ball and a separate string attached to the back of the car. When he is satisfied that all adjustments are correct and that the ball and the car are *at the same distance d* from their proposed point of meeting C, he releases them. The distance d which car and ball travel may be varied, but is, of course subject to local limitations of space, both horizontal and vertical.

The rapid acceleration of the car is impressive, and the "catch" which it makes in passing beneath the ball is a neat trick. The motion is violent and the experiment is likely to end in damage to apparatus or furniture unless means are provided for stopping the car before it reaches the end of the table. Various methods come to mind, but perhaps the simplest is to have an assistant hold the end of a slack string attached to the back of the car and of such length

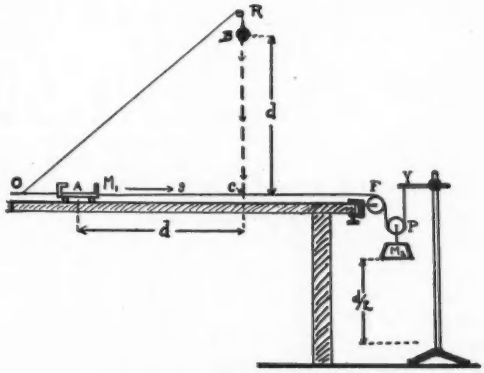


FIG. 2. The experimental arrangement.

that the slack is taken up soon after the car passes the point where it catches the ball. This has the advantage that there is no rebound of the car, such as might follow the use of a strong buffer spring to stop the car. It is suggested that a cushion be placed on the floor beneath M_2.

In computing the necessary mass ($M_2=4M_1$) no account is taken of friction, inertia of wheels or pulleys, or of the weight of the movable pulley P. If greater precision is desired, as in a laboratory experiment, these factors can be considered in the usual manner, either by computation or by experimental adjustment. However, in the arrangement tested, the weight of the movable pulley was sufficient to take care of friction and rotary inertia of pulleys and wheels, and the ratio of masses, M_2/M_1, was made just 4.

For laboratory measurements, the box on the car may be replaced by a horizontal board covered with sensitized paper which will register the point of impact of the dropping ball. By this means it is possible to obtain a direct measurement of the distance traversed by the car, with any appropriate acceleration a greater or less than g, during the time required for the ball to fall through a measured distance. It is thus possible to check the values given in Table I. Simple analysis shows that the ratio of the horizontal distance traversed by the car to the vertical distance fallen by the ball is equal to the ratio a/g. It is evident that a, the acceleration of the car, may be maintained virtually constant, barring mechanical defects, over a distance equal to twice the distance of descent of the mass M_2.

In the second column of Table I are given the accelerations of M_1 for the three arrangements shown in Fig. 1. If these three accelerations are plotted as functions of the ratio M_2/M_1 (Fig. 3), several interesting points are brought out. First, there is no categorical answer to Question 1, for when M_2 is small (less than $\frac{1}{2}M_1$), the advantage lies with Case C; then Case A takes the lead for

TABLE I.

Case	Acceleration a of M_1	Max. Acceleration ($M_2 \gg M_1$)	f_s	f_d
A	$\frac{M_2 g}{M_1+M_2}$	g	$M_2 g$	$M_2(g-a)$
B	$\frac{2M_2 g}{4M_1+M_2}$	$2g$	$\frac{1}{2}M_2 g$	$\frac{1}{2}M_2(g-\frac{1}{2}a)$
C	$\frac{2M_2 g}{M_1+4M_2}$	$\frac{1}{2}g$	$2M_2 g$	$2M_2(g-2a)$

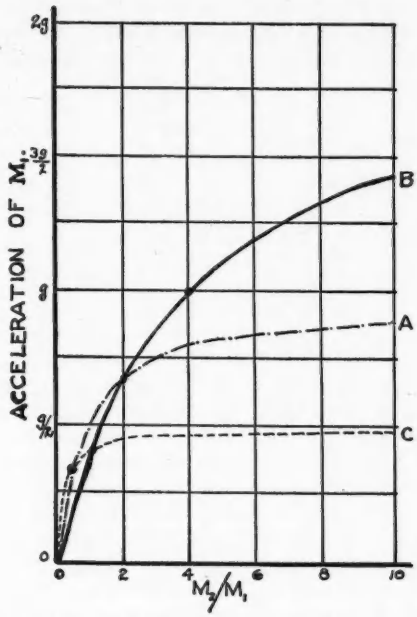


FIG. 3. Acceleration a of the car as a function of the ratio M_2/M_1.

values of M_2 between $\frac{1}{2}M_1$ and $2M_1$, but Case B has the advantage for larger values of M_2. Second, Case B is the only one capable of fulfilling the requirement of Question 2; it is evident from Table I that when M_2 is equal to $4M_1$, the ratio $2M_2/(4M_1+M_2)$ becomes equal to unity. Finally, the maximum acceleration obtainable (for $M_2 \gg M_1$) approaches g, $2g$ and $\frac{1}{2}g$, as shown in the third column.

Perhaps no other experiment in the whole gamut brings out so clearly the difference between the forces that act on a system when it is at rest and when it is accelerated. Paradoxically, the system C, which has the largest "static" mechanical advantage, from the point of view of the force applied to M_1, provides the smallest ultimate acceleration; whereas the system with the smallest mechanical advantage (B) is the only one capable of exceeding g. These facts are emphasized in the last two columns, in which f_s represents the "static force" in the cord attached to M_1 when each system is at rest, and f_d represents the "dynamic force" when the acceleration of M_1 is a.

The extension of this method to obtain still greater accelerations is obvious. It is only necessary to use a system like Case B but with a larger "mechanical disadvantage." If one inverts the

use of an ordinary block whose mechanical advantage is n, it may be used to produce a horizontal acceleration a given by the equation

$$a=nM_2g/(n^2M_1+M_2).$$

For example, with a six-strand tackle ($n=6$), it would be possible to obtain an acceleration of $2g$ if M_2 were made equal to $18M_1$; and, of course, M_2 would move only one-sixth as far as M_1.

Rolling Spheres and Cylinders

W. B. PIETENPOL
University of Colorado, Boulder, Colorado

THE demonstration of moment of inertia by cylinders that undergo different accelerations down an inclined plane is an experiment of interest and value to beginners in physics.[1] Both cylinders and spheres of special design, incorporating equal masses, equal external dimensions, and a maximum difference in moments of inertia, have been used by the writer for many years. The specifications imposed lead to interesting results. A maximum difference in accelerations is thus produced, and the extension of fundamental ideas should make the problem one of value to students in mechanics.

Let us first consider the design of two spheres made of materials of densities d_1 and d_2 such that the two spheres will have equal masses, equal external diameters, and a maximum difference in their moments of inertia.

Figure 1 represents the cross sections of the two spheres, (a) and (b), with radii and densities as indicated. For convenience in this analysis we may consider the density d_1 greater than d_2.

The masses of the two spheres are

$$M_a=\tfrac{4}{3}\pi r_1^3d_2+\tfrac{4}{3}\pi(R_1^3-r_1^3)d_1,$$
$$M_b=\tfrac{4}{3}\pi r_2^3d_1+\tfrac{4}{3}\pi(R_2^3-r_2^3)d_2.$$

Now let the two spheres have equal external diameters and equal masses; then

$$R_1=R_2=R, \tag{1}$$

and

$$r_1^3d_2+R^3d_1-r_1^3d_1=r_2^3d_1+R^3d_2-r_2^3d_2,$$
$$r_1^3(d_2-d_1)+r_2^3(d_2-d_1)=R^3(d_2-d_1),$$

or

$$r_1^3+r_2^3=R^3. \tag{2}$$

This relationship, that the sum of the cubes of the radii r_1 and r_2 must be a constant and equal to the cube of the external radius R, is seen to be independent of the densities d_1 and d_2.

Consider now the moments of inertia of the two spheres. The moment of inertia of a sphere of mass m and radius a about any diameter is

$$I=(2/5)ma^2=(8/15)\pi a^5d.$$

The moment of inertia of a spherical shell about any diameter, where d is the density and a_1 and a_2 are the two radii, is

$$I_s=(8/15)\pi(a_1^5-a_2^5)d.$$

Therefore, the moments of inertia of spheres (a) and (b) about their diameters are

$$I_a=(8/15)\pi r_1^5d_2+(8/15)\pi(R_1^5-r_1^5)d_1,$$
$$I_b=(8/15)\pi r_2^5d_1+(8/15)\pi(R_2^5-r_2^5)d_2.$$

Now for the condition represented by Eq. (1), the difference in the moments of inertia of the two spheres becomes

$$I_a-I_b=(8/15)\pi[r_1^5(d_2-d_1)+r_2^5(d_2-d_1)-R^5(d_2-d_1)]$$
$$=(8/15)\pi(d_2-d_1)(r_1^5+r_2^5-R^5).$$

Introducing now the restriction imposed previously, that the spheres have equal masses [Eq. (2)], the expression becomes

$$I_a-I_b=(8/15)\pi(d_2-d_1)\times[(R^3-r_2^3)^{5/3}+r_2^5-R^5]. \tag{4}$$

To obtain an expression for the maximum difference in the moments of inertia of the two spheres, differentiate both members of Eq. (4) with respect to r_2 and equate to zero:

$$\frac{d(I_a-I_b)}{dr_2}=\tfrac{8}{15}\pi(d_2-d_1)\times[-\tfrac{5}{3}(R^3-r_2^3)^{\frac{1}{3}}3r_2^2+5r_2^4]=0$$

from which $2r_2^3=R^3$, or $r_2=R/1.25992$. But

$$r_1^3+r_2^3=R^3=2r_2^3.$$

Thus the following relationship must exist:

$$r_1=r_2=0.7937R.$$

Thus for the two spheres of equal masses and equal external radii, the internal radii must be

[1] Sutton, ed., *Demonstration experiments in physics*, "Racing rollers," p. 68.

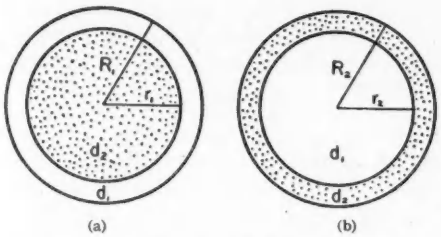


FIG. 1. Cross sections of the two spheres.

equal in order that there may be a maximum difference in moments of inertia. This difference is also directly proportional to the difference in densities of the materials of which the spheres are constructed, as was to be expected.

In the experiment of accelerated motion down an inclined plane the torques acting on the two spheres of equal masses are the same since the centers of mass are at the geometric centers of the spheres. Furthermore, the moments of inertia of the spheres about the points of contact in both cases involve, by Steiner's theorem, the added constant quantity MR^2. Thus on the assumption that there is no slipping, the difference in the angular accelerations is a maximum for the spheres designed in the afore-described manner.

A similar analysis shows that for two cylinders of equal lengths, equal masses, and equal external diameters,

$$r_1^2+r_2^2=R^2$$

and, for maximum difference in their moments of inertia,

$$r_1=r_2=0.7071R.$$

The problem may readily be extended to include calculations of momentums or kinetic energies of rotation and translation at different times or positions.

A valuable variation of the experiment is the use of a curved track instead of an inclined plane. If the track has double grooves to constrain the motion of the spheres, and is extended so the spheres will rise on the opposite side, another fundamental principle is illustrated. The oscillation periods are strikingly different, and while the velocities of the spheres at the lower part of the track vary greatly, the two spheres by the law of conservation of energy rise to the same height on the opposite side of the track insofar as frictional loss may be neglected.

The construction of the spheres and cylinders described offers no great difficulty. Obviously cylinders are relatively easy to build as they may be turned on the lathe to a tight fit or cemented together if necessary. The writer uses pine wood and steel for the two materials, the cylinders having an approximate length of 6 cm and an external diameter of 10 cm. The use of cylinders is desirable in that the construction is apparent to the observer. Spheres are somewhat more difficult to build since hemispherical shells need to be turned on the lathe and then cemented together over the central sphere. Spheres of approximately 6 cm external diameter are very satisfactory. Work can be saved by selecting a steel ball commercially available, for the core of one of the spheres. The use of spheres, to all appearances balls of different materials, introduces the element of surprise which is sometimes of value.

An Experiment on Angular Simple Harmonic Motion

JOSEPH W. ELLIS
University of California, Los Angeles, California

FOR use as a laboratory experiment in an introductory course limited to mechanics we have devised an experiment on angular simple harmonic motion that is analogous to the one on linear simple harmonic motion involving the vertical vibration of a loaded spiral spring. The two experiments are performed in one 2-hr laboratory period.

The new experiment involves an adaptation of the rotational inertia apparatus supplied by the Central Scientific Company and shown in Fig. 1. Two strings are wound in opposite directions around one of the disks of the compound rotor, and are led off vertically to two parallel spiral springs suspended from a horizontal bar. The rotor comes to rest when the forces in the two springs are equal. When the rotor is turned through approximately one revolution and released, it oscillates with angular simple harmonic motion provided Hooke's law is applicable to the springs over the range of operation and provided the bearing friction is negligible. One may

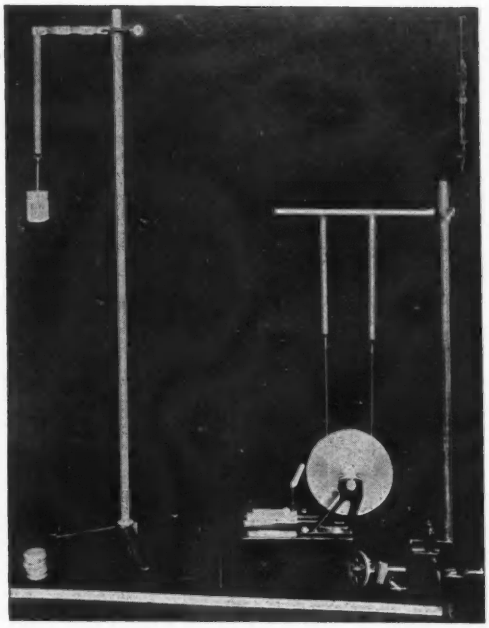


FIG. 1. Experiments on linear (left) and angular (right) simple harmonic motion.

reasonably assume that Hooke's law holds for any well-wound spring. We did have difficulty, however, with bearing friction, and it was only after ball bearings had been introduced that the experiment worked with any degree of satisfaction.

The bearings selected were SKF extra-small ball bearings No. 36-1Z. The outer race of a bearing was inserted into, and flush with, each end of the rotor shaft and was held with three screws inserted longitudinally into the shaft. The inner race was supported by the cone of the screw that formed the original pivot bearing.

Since the new experiment is to be carried through analogously to the first part with the vibrating loaded spring, a good direct determination must be made of the period of oscillation T and this must be compared with the value obtained from the equation

$$T=2\pi\sqrt{(I/L_0)}. \qquad (1)$$

When the rotor is given an initial angular displacement of a half revolution or more with the hand, it oscillates at least 20 times without serious diminution of amplitude. Hence, from several such trials a good average, direct determination of T can be made.

To use Eq. (1) it is necessary to know the moment of inertia I of the rotor and the restoring torque per unit angle, L_0, for the particular arrangement involved. The value of I is either computed from the mass and dimensions of the rotor or is determined in an earlier experiment involving continuous rotation of the rotor. In that experiment the rotor is mounted as shown in Fig. 2, some 2 m above the floor. A string is wound either about the shaft or about one of the large disks of the rotor, and a mass is attached and allowed to descend under gravity. From the magnitude of this mass, and its distance and time of descent the moment of inertia I is determined.

With the apparatus arranged as in Fig. 1, the restoring torque L_0 is determined by wrapping an additional string about the shaft or one of the disks and leading it off horizontally over a ball-bearing pulley clamped to the edge of the table. This string is loaded with a weight sufficient to make the rotor turn through an angle of at least 1 rad. To measure this angle, a paper disk, calibrated in degrees and supplied by the manufacturer for use in another way with this rotor, is cemented on the flat face of the largest disk. The angular position is indicated by a sharp pointer

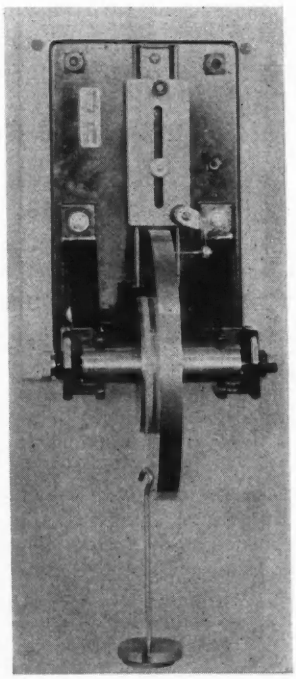


FIG. 2. Method of determining the moment of inertia of the rotor.

which replaces the glass capillary that was a part of the original sparking mechanism.

Although the experiment is to be performed as just outlined, in the following data the moment of inertia has been solved for by substituting observed values of T and L_0 in Eq. (1). When the strings attached to the springs were wound about the smaller of the two disks, and the torque constant L_0 was determined by winding the auxiliary string about the shaft, the data obtained by a competent graduate student yielded 2.28×10^5 g cm^2 for I. This is to be compared with 2.31×10^5 g cm^2 determined from the mass and dimensions of the rotor, and with the following values determined from the continuous rotation of the rotor: (1) 2.36×10^5 g cm^2 when the string was attached to the shaft; (2) 2.30×10^5 g cm^2 when it was attached to the smaller disk; (3) 2.32×10^5 g cm^2 when it was attached to the larger disk.

The experiments described here are followed by one on the torsion pendulum. The latter experiment is regarded not as one for the verification of the fundamental equation for the period of an angular simple harmonic motion but as presenting a standard method for the comparison of moments of inertia. Thus we determine the moment of inertia of a disk about a central axis from the known value for a ring; and then determine the value for another disk about an axis at the edge of the disk and perpendicular to the disk from the previously determined value for the original disk by using the Lagrange theorem, $I=I_c+Mr^2$.

A Centripetal Force Experiment

W. C. Kelly and E. S. Messer
University of Pittsburgh, Pittsburgh, Pennsylvania

THIS paper describes a centripetal force experiment that utilizes standard apparatus in a direct weighing method yielding results of satisfying accuracy. The use of standard apparatus is an important consideration in general physics laboratories where many setups of the same experiment are required. The method is such that the student can investigate the separate dependence of the centripetal force upon any one of the three quantities—the mass of the body, its velocity, and the radius of curvature of the path.

Some centripetal force experiments require expensive apparatus. Others use simple equipment, but are not easy to interpret. In the conical pendulum experiment—as used at the University of Pittsburgh—one measures the period of a pendulum bob moving in a horizontal circle of known radius. The apparatus is simple, the results accurate, and the application of the underlying vector mechanics excellent. However, the beginning student encounters difficulty in interpreting the results of the experiment because the independent variables—velocity and radius of path—cannot be varied separately with convenience. Instead of finding the inverse relation between centripetal force and radius which is so much stressed in class in discussing the equation $F=mv^2/r$, the thoughtful student is troubled to find that the centripetal force apparently varies directly with the radius.

Using apparatus largely shop-built, P. H. Miller, Jr. has devised an experiment[1] yielding reasonably accurate results on the basis of theory similar to that described here; his procedure, however, is less direct than the present one.

Our experiment consists in measuring the force exerted on a ball swinging as a pendulum when the ball passes through the point of zero displacement, at which point the velocity can be readily computed. Consider (Fig. 1) a pendulum bob of weight W swinging downward from rest at an elevation h_2 above an arbitrary level in a vertical circle of radius r. If the vertical position of the ball as it passes through the zero point is h_1, its velocity v here is

$$v=[2g(h_2-h_1)]^{\frac{1}{2}}. \qquad (1)$$

Then the centripetal force F acting on the ball at this point is given by

$$F=Wv^2/gr=2W(h_2-h_1)/r. \qquad (2)$$

The force exerted on the ball by the suspension cord at this point is the sum of the centripetal force and the weight of the ball. Hence, if one measures by means of an ordinary trip scale this force and the weight of the ball separately, he can determine the centripetal force as the ball passes through the zero point. Knowing the radius of the path and the heights h_2 and h_1, one can

[1] P. H. Miller, Jr., Am. J. Phys. **12**, 40 (1944).

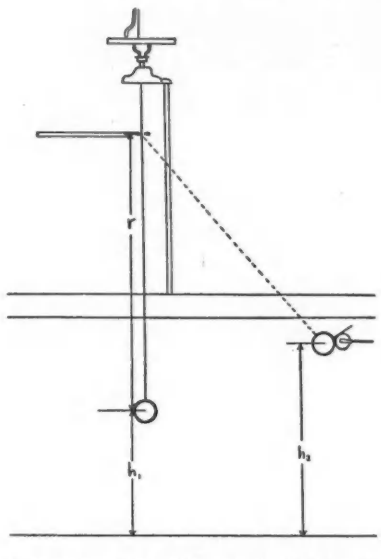


FIG. 1. Radius r and difference in heights h_2 and h_1 determine centripetal force as ball swings through point of zero displacement.

compute from Eq. (2) a value of the centripetal force for comparison with the experimental value.

The apparatus is shown in Fig. 2. The essential parts are (1) the trip scale mounted at the top of a rigid 10-mm rod, (2) a ball suspended by a light strong cord from the hook beneath the left-hand pan of the scale, and (3) a slot through which the cord passes. Accessory apparatus includes a board with leveling screws, to provide a base for measuring vertical distances; a pulley mounted on a stand, for releasing the ball; a meter stick; and standard weights. The pointer of the scale is lengthened by a straight wire moving over a white card as background so that small deflections can be detected. A screw clamp is placed beneath the right-hand pan of the scale to keep the pointer at zero when the load on the right-hand pan exceeds that on the left.

The scale is a standard trip scale; it is convenient for the sliding weights to be adjustable from zero to 210 gwt. The supporting rod has been made very rigid. Two crossbars and a third bar at right angles to them were sufficient in our case. The ball weighs about 500 gwt and has a horizontal pointer to indicate its center of mass; it is suspended by a braided fish line of diameter about 0.5 mm and length about 1 m. The slot is formed by two thin metal strips separated by 0.8 mm and soldered to a rod.

With the ball hanging freely at rest, the slot is placed perpendicular to the plane of motion and centered. The ball is then drawn back into contact with the pulley (see Fig. 1) by a piece of cord and the height of the pulley adjusted until the cord is perpendicular to the suspension. To determine the initial position of the ball, one measures from the pointer indicating the center of mass to the leveled board. Some load in excess of the weight of the ball is then placed on the right-hand pan of the scale, and the ball is released. The best method of release is a forward motion of the hand while relaxing one's hold on the cord—a "pushing on the cord," as it were. One observes

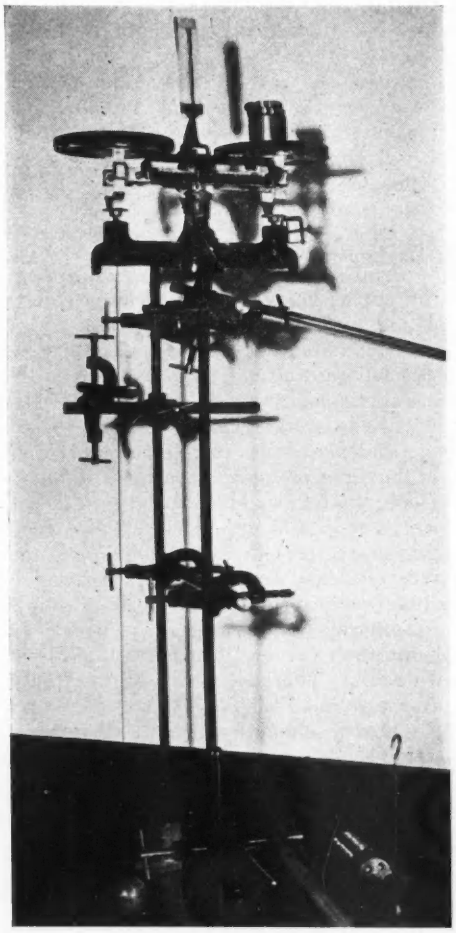


FIG. 2. Showing trip scale mounted on well-braced vertical rod.

TABLE I. Typical data for a ball weighing 511.8 gwt.

Quantity	Test 1	Test 2
h_1 (cm)	35.00	35.30
h_2 (cm)	79.70	51.95
r (cm)	93.30	93.05
Load (gwt), just no motion	996.9	696.6
F (gwt), experimental	485.1	184.8
F (gwt), computed	490.4	183.2
Difference (percent)	1.1	0.9

the motion of the pointer on the scale as the ball swings through its zero point. No deflection indicates that the load on the right-hand pan exceeds the sum of the centripetal force and the weight of the ball; a deflection indicates that the load is less than that sum. In repeated trials, releasing the ball from the same position, the experimenter varies the load until the pointer *just fails* to deflect. Experience has shown that as small a change as 0.5 gwt in 800 gwt can be detected. The weight of the ball subtracted from this balance load gives the experimental value of the centripetal force.

In determining the radius and the vertical position of the ball at the zero point, one should take into account the stretching of the suspension cord. This may be done by loading the ball until the tension in the cord about equals that existing when the ball experienced the observed centripetal force. With the pointer of the scale at zero, one measures r and h_1. Equation (2) is then used to compute the force F. Some typical data are given in Table I.

It will be noticed that by releasing the ball from the same vertical position and varying the position of the slot in succeeding determinations, the dependence of the centripetal force upon the radius of the path may be studied while the velocity remains constant. A second part to the experiment would consist in keeping the radius fixed and varying the velocity by varying the initial height.

An agreement of 1 percent seems typical, giving an accuracy about equal to that of the conical pendulum experiment and somewhat better than that claimed by Miller.[1] An even better recommendation for the experiment is the directness of the method.

RECENT MEETINGS

Western Pennsylvania Chapter

The Association of Physics Teachers of Western Pennsylvania and Environs met at Carnegie Institute of Technology on April 14. There were both morning and afternoon sessions. The program follows:

Reports on the January meeting of the Association. A. G. WORTHING, O. H. BLACKWOOD AND W. C. KELLY. *University of Pittsburgh,* AND W. H. MICHENER AND C, WILLIAMSON, *Carnegie Institute of Technology.*

A method of presenting accelerated motion. MICHAEL SERENE, *Ambridge High School.*

Student laboratory projects. HARRY HILL, *Washington and Jefferson College.*

The molecular ray experiments of Otto Stern. S. N. FONER, *Carnegie Institute of Technology.* This was followed by a visit to Doctor Stern's laboratory.

Display and demonstrations of apparatus for laboratory and lecture: (*a*) Apparatus for measuring torque; Large working model of the eye. W. N. ST. PETER, *University of Pittsburgh.* (*b*) Pascal's principle and variation of pressure with depth. A. G. WORTHING, *University of Pittsburgh.* (*c*) A two-fluid barometer; Linear expansion apparatus with special pump for circulating water; Two types of apparatus for measuring thermal conductivity. C. WILLIAMSON, *Carnegie Institute of Technology.*

The officers for the coming year are: *President,* W. H. Michener; *Vice President,* Harry Hill; *Secretary,* Chas. Williamson. O. H. Blackwood was elected for a three-year term starting in 1946 as representative of the chapter on the Executive Committee of the Association.

W. H. MICHENER, *Secretary*

Southeastern Section

THE eleventh annual meeting of the Southeastern Section of the American Physical Society was held on a restricted basis in the Atlanta Biltmore Hotel and at the Georgia School of Technology, Atlanta, Georgia, on April 6 and 7, 1945. Approximately 50 members and guests attended, 34 being from out of town. Local arrangements were made by a committee headed by E. E. Bortell.

The regular program consisted of ten papers, abstracts for seven of which appear below. Abstracts of the other papers appear in the May 1 and 15, 1945 issue of the *Physical Review.*

A feature of the program was a *Symposium on high polymers,* at which the following papers were presented: "The chemical characterization of high polymers," Kyle Ward, Jr.; "X-ray diffraction as applied to the study of semicrystalline materials," R. C. L. Mooney; "The relation of physical properties to the molecular character of high polymers," Emil Ott. Additional features were a review of the last annual meeting of the American Association of Physics Teachers, by D. R. McMillan, and an informal panel discussion, following the annual dinner, on the general topic of the training of physicists for work in industry.

At the business meeting the election of the following

officers was announced: *Chairman*, F. L. Brown; *Vice Chairman*, R. C. L. Mooney; *Secretary*, E. Scott Barr; *Treasurer*, C. B. Crawley; *Member of Executive Committee*, A. E. Ruark.

1. Reflections from two plane mirrors. Frederick L. Brown, *University of Virginia*.—The complete paper will appear in the next issue.

2. Demonstration of the Peltier effect. Robert W. Koza, *Georgia School of Technology* (*Introduced by J. H. Howey*).—A straight copper wire, about 1 mm in diameter, with a 5-cm long constantan insert of the same diameter, is stretched between two supports in such a way that a current can be passed through it. The Peltier effect is demonstrated by the temperature difference produced between two copper-constantan junctions when a direct current is passing through this heater wire. This temperature difference is measured by an electrically independent thermocouple circuit. The two junctions of the measuring couple are mounted near the junctions of the heater wire by attaching them to thin, cylindrical, copper shells, 3 mm in diameter and 1 cm long, mounted coaxially around each of the junctions of the heater wire. The deflection of a galvanometer in the measuring couple circuit is approximately proportional to the magnitude of the Peltier effect. Any asymmetrical Joule heating may be balanced out by small axial motion of the heater wire relative to the copper shells. By mounting the heater wire on flat springs, this axial motion is secured, without loss of tension and subsequent bending, by the action of a screw against one of these mounting springs.

3. An Apparatus for the measurement of vapor pressures. Alvin W. Hanson, *The Citadel*.—The apparatus is a modification of the conventional static vapor-pressure apparatus for student use, easily constructed of standard laboratory supplies without using glass seals or a vacuum pump. The range of measurement is from 2 to 960 mm-of-mercury absolute. The accuracy is within 1 percent. A 200-ml round bottom, short ring-neck boiling flask, in upright position, containing about 100 ml of mercury, is provided with a 3-hole stopper to admit a thermometer, a short glass tube with a stopcock and a glass tube connecting the mercury pool in the flask to an open mercury manometer. The rubber stopper and tubes are sealed with red sealing wax. A continuous mercury column extends from the open side of the manometer to the closed side and over a siphon bend into the mercury in the flask. The liquid to be tested is poured through the stopcock to form a layer on the mercury pool. The flask is heated in a bath to boil the liquid long enough for the vapor to purge the air through the stopcock. When the stopcock is closed the vapor pressure may be found from the manometer readings.

4. Physics: too little and too late. K. L. Hertel, W. R. Rusk, and Mary Peters, *University of Tennessee*.—It is generally agreed that students entering college physics are ill-prepared and usually have many misconceptions of physical phenomena. The various training programs of the armed forces also have drawn attention to the wide-spread deficiency in the knowledge of physical science. In general, people do not have a sufficient mastery of scientific principles to lead a well-rounded life in our present civilization. The prewar indifference to training in mathematics and the physical sciences, particularly at the lower levels, is mainly responsible for this condition. It has been presumed that the entire responsibility for the formulation of elementary and secondary school programs has rested upon the school administrators along with state departments of education and the teacher training divisions of our colleges. Perhaps it is time for physicists to discard this presumption, offer to assume a substantial share of the responsibility with those in the field of education, and become deeply concerned with the teaching of basic physical principles at all levels of instruction to the end that physics shall make a more significant contribution to our national life.

5. Report on the nature and progress of a study of the problems of instruction in the natural sciences in Southern Universities and Colleges. E. H. Dixon, *University of Georgia*.—This study is being sponsored by the University of Georgia in cooperation with other universities and colleges of the South; G. H. Boyd, Dean of the Graduate School, University of Georgia, is directing the study. It will consist of: (1) a survey and evaluation of present practices in science instruction; (2) recommendations for the improvement of science teaching in the South; (3) recommendations for the improvement of the position of science in the educational program of the South; (4) statements presenting points of view on vital topics.

6. The nature of biological action of radiations of short wavelength. A. A. Bless, *University of Florida*.—There seems to be no single theory capable of explaining all the observed effects of x-rays and gamma-rays. The genetic effects of these radiations are adequately explained by the "all-or-none" hypothesis. However, the exact nature of the action is not clear. Some of the inhibition effects which are not genetically transmitted are best explained on the basis of the "poison" theory, while other effects seem to fit the "hit" theory. There is some evidence that the action of radiation takes place through the medium of enzymes.

7. The nomographic representation of polynomials. W. H. Burrows, *Georgia School of Technology* (*Introduced by G. A. Rosselot*).—Standard methods for the nomographic representation of polynomials in three and four variables are usually given for very limited ranges of the variables. The use of ranges frequently encountered in engineering practice makes these methods inaccurate and difficult to employ. A chart has been devised for adjusting the moduli of the scales to fit the ranges of the variables, at the same time fixing the positions of points on the base scale of the nomograph. Its use was described in Part I, while the method as a whole was reduced to simple steps applicable to all nomographs of not over four variables. In Part II, two methods for the treatment of polynomials of five variables were given. Previous methods require the reduction of the polynomial to one of four variables, which is indirect, or construction of a complicated network of curves. The methods here given are direct and follow the simple methods outlined in Part I.

E. Scott Barr, *Secretary*

NOTES AND DISCUSSION

A Problem in Psychophysics

PAUL KIRKPATRICK
Stanford University, Stanford University, California

PROFESSOR Watson's fine series of pictures relating to the history of physics[1] has reminded me of the illustration of a great moment in science that the late Professor P. A. Ross executed and used as a bookplate.

From the trajectory of the apple it should be possible for any reader to calculate an upper limit for the great man's reaction time.

[1] E. C. Watson, Am. J. Phys. **13**, 48 (1945).

A Model to Demonstrate Spherical Aberration of a Concave Spherical Mirror

F. R. HIRSH, JR.
University of Southern California, Los Angeles, California

SPHERICAL aberration is a simple phenomenon with an especially forbidding title. It is easily explainable with the aid of a simple diagram (Fig. 1) which is seen in many physics textbooks. The spherical mirror is in section an arc of a circle AO with center at C, on which a light ray, parallel to the mirror axis OFC, is incident at A. The reflected light ray AF makes an angle $r(=i)$ with the normal to the mirror at A, and it is obvious that the triangle AFC

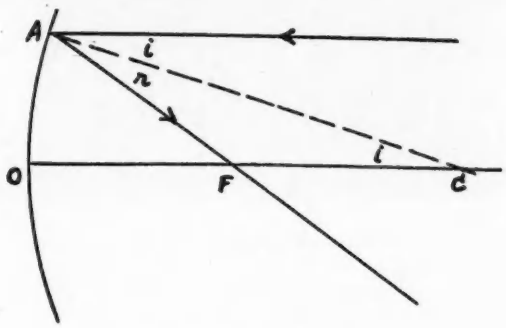


FIG. 1. Concave spherical mirror.

is isosceles, with the legs AF and FC equal. Now as the incident ray is moved closer to the mirror axis, the isosceles triangle flattens out and the image distance $OF=OC-FC$, increases since FC decreases continuously. In the limit for a paraxial ray $OF=\frac{1}{2}R$, $(OC=R)$. Hence there is no single image, but each parallel ray gives its own image. This is the phenomenon of spherical aberration.

Figure 1 suggested the working model shown in Figs. 2 and 3. In Fig. 2 an incident ray parallel to the axis is represented by the white wooden bar at the back of the model on which is a black arrow pointing to the left. At the left the white (plywood) circular track represents the mirror central section. Pivoted on the left-hand end of the incident ray is the radius of curvature (dark colored) which is also pivoted at the right-hand end at the continuation of the horizontal white track, the mirror axis. The pivoted end of the incident ray is guided by a carriage running on two spool halves on the mirror section track. The incident ray's right-hand end slides through a pierced block which in turn slides on a white wooden track at right angles to the mirror axis; thus the incident ray is kept parallel to the mirror

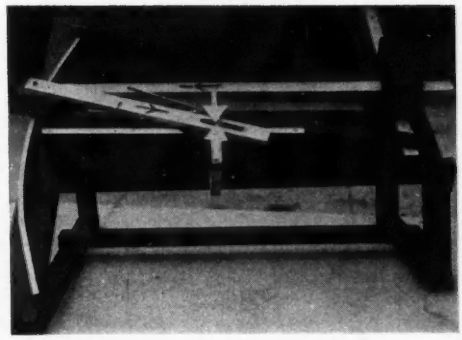


FIG. 2. Photograph of the model, showing the image position for an incident ray close to the mirror axis.

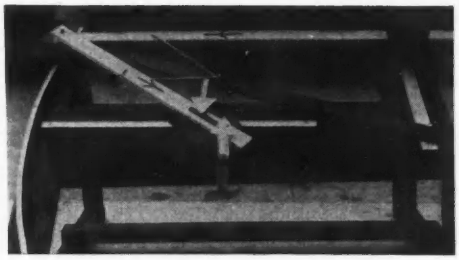


FIG. 3. Image position for an incident ray far from the mirror axis.

axis. The reflected ray, the diagonal white slat with the black arrow pointing down to the right, is kept at the proper angle $r(=i)$ with the radius of curvature slat by two heavy wires of equal length fastened at equal distances from the pivot on the incident and reflected rays and wired together at the ends, the fastened ends moving together in a slot in the radius of curvature slat.

Figure 2 shows the image position (white arrows) for an incident ray close to the mirror axis. The lower white index arrow is actually fixed at the position $f=\frac{1}{2}R$ for a paraxial ray. Figure 3 shows the image position (top index arrow pointing down) for a ray relatively much farther from the mirror axis, while the paraxial ray focus is still represented by the white index arrow pointing up. The top index arrow is on a peg guided by two spool halves along the mirror axis track while the peg is pulled along by the reflected ray slat, as the incident ray is moved out from the mirror axis. As the incident ray is moved out from the mirror axis (and kept parallel to it), by *moving* up the *pivot peg* at the left-hand end of the incident ray, and the *pierced block*, the two index arrows move smoothly apart showing the continuous change of image position (upper arrow); that is, the model demonstrates spherical aberration.

The model is about 2 ft in length. It was made in a few hours from scrap lumber and offers a clear explanation of the phenomenon of spherical aberration. I wish to thank Mr. Till of our staff for aid in constructing the model.

Some Diophantine Equations

R. T. LAGEMANN
Emory University, Emory University, Georgia

MANY teachers advocate the use of integral numbers in physics problems. Particularly on examinations does this view seem justified, for one wishes to test ability in physics and not ability in working with irrational numbers or rational fractions. To satisfy this need partially, tables of such integers for use in the lens equation and in solving right triangles have appeared in this journal,[1,2] and Dorwart[3] has pointed out that Dickson[4] had published the solution of the lens equation.

For those desiring to use integers in some other relations, seven Diophantine equations including the two published earlier are listed with their solutions in Table I. In general,

TABLE I. Integral solutions of some equations used in physics.

Equation	Solutions
$x^2+y^2=z^2$	$x=2pqr$ $y=r(p^2-q^2)$ $z=r(p^2+q^2)$
$x^2+y^2+z^2=t^2$	$x=2(p^2+q^2-r^2)$ $y=2\{(p-r)^2-q^2+p(q-r)\}$ $z=(q-r)^2-p^2+4q(p-r)$ $t=3\{(p-s)^2+q^2\}+2r(p-q)$
$x^3+y^3+z^3=t^3$	$x=6p^2qr+q(q\pm p)s+3q(q\mp p)r^2$ $y=6p^2qr-q(q\pm p)s-3q(q\mp p)r^2$ $z=-6q^2pr+p(p\pm q)s+3p(p\mp q)r^2$ $t=6q^2pr+p(p\pm q)s+3p(p\mp q)r^2$ where $s=p^4+p^2q^2+q^4$
$\frac{1}{x}+\frac{1}{y}=\frac{1}{z}$	$x=p(p+q)r$ $y=q(p+q)r$ $z=pqr$
$\frac{1}{x}+\frac{1}{y}+\frac{1}{z}=\frac{1}{t}$	$x=qr(p+q+r)k$ $y=pr(p+q+r)k$ $z=pq(p+q+r)k$ $t=pqrk$
$\frac{1}{x^2}+\frac{1}{y^2}=\frac{1}{z^2}$	$x=(p^2+q^2)(p^2-q^2)r$ $y=2pq(p^2+q^2)r$ $z=2pq(p^2-q^2)r$
$\frac{1}{x^2}+\frac{1}{y^2}=\frac{1}{x_1^2}+\frac{1}{y_1^2}$	$x=q(p^2-q^2-r^2)(p^2-q^2+r^2)k$ $y=p(p^2-q^2-r^2)(p^2-q^2+r^2)k$ $x_1=2pqr(p^2-q^2-r^2)k$ $y_1=2pqr(p^2-q^2+r^2)k$

p, q and r are any integers, and k, when used, is a factor chosen to make integers of x, y, z and t. This table should yield ample values for the physics teacher. To secure all the irreducible solutions without duplication, additional conditions would have to be imposed, and for those the accompanying references may be consulted. The first five solutions are from Dickson.[4] The last is from Gronwall.[5] For the solution to the sixth I am indebted to Dr. C. E. Clark[6] of this university.

[1] R. H. Bacon, Am. J. Phys. (Am. Phys. T.) **4**, 197 (1936).
[2] R. M. Bell and M. G. Zabetakis, Am. J. Phys. **12**, 231 (1944).
[3] H. L. Dorwart, Am. J. Phys. **13**, 54 (1945).
[4] L. R. Dickson, *History of the theory of numbers* (Carnegie Institution of Washington, 1920), vol. 2.
[5] T. H. Gronwall, Phys. Rev. **36**, 1671 (1930).
[6] C. E. Clark, private communication.

WE need education in the obvious more than investigation in the obscure.—OLIVER WENDELL HOLMES.

DIGEST OF PERIODICAL LITERATURE

Selection and Training of Students for Industrial Research

Ordinarily we do not print extensive digests of material in readily available periodicals. However, it is believed that the present article should be made available not only to physics department staffs but, through departmental libraries, to all students who major in physics. The article was originally planned for publication in the *American Journal of Physics* but, upon completion, proved to be so widely applicable that it was placed in a periodical devoted to the sciences in general.—EDITORS.

The qualities sought in selecting college graduates for industrial research may be classed in four groups. They are, in order of importance, *character*, *aptitude for research*, *attitude toward work* and *knowledge*. Not all of these qualities are the product of college training, but many of them might be.

Character is rated first in the hiring and retaining of personnel. The many virtues which it comprises include self-discipline, courage, tolerance, honesty and generosity. Honesty is more than the negative virtue of not telling lies. It is also the quality that enables one to say of a man, "I always know just where to find him." The basis of true friendship and teamwork, it is an essential requirement in a cooperating group. Moreover, the honest research worker avoids deceiving not only others but himself. The greatest scientific sin—wishful interpretation of data—is more common than is generally realized, and is so serious that it can make a worker valueless. The man who is more anxious to prove that he is right than to find the truth will nearly always arrive at a conclusion in accordance with his wishes. Observations will be shaded, without intentional deceit, to agree with preconceived ideas, and things may even be seen that are not there. The habit of scientific honesty, of facing facts impartially, is more important than knowledge to a research worker.

Generosity, an old-fashioned virtue, is essential for cooperation, without which a laboratory today is primitive. One or two ungenerous men can poison mutual confidence in a laboratory. Jealousy, greed and suspicion are weeds that grow easily, while cooperation is a delicate flower. Sometimes a person of exceptional qualifications can be treated as a prima donna, but usually no laboratory can afford to hire even a genius who lacks the generous spirit of cooperation.

If character is the most important research qualification, then it should be a prime educational objective. All colleges would say it is, but would have to admit that the fraction of educational effort put on it is small, because of lack of an accepted method of character training. Admittedly it is the most difficult as well as the most important of educational tasks. But when have universities shrunk from a task because it is difficult?

What is needed is emphasis. War has taught us that seemingly impossible tasks can be accomplished if given sufficient priority. Although this problem is under study, and eventually may be better understood, we could, in the meantime, make more use of what psychology has already shown. Character is neither inherited nor taught by precept; it is the product of self-discipline under the stimulus of the predominant environment, which today is the school. Therefore, character should be the first requirement in choosing teachers and students. We are "molded to manhood" under the influence of teachers and fellow students. Industrial laboratories put more emphasis on character in selecting personnel than colleges do. It ought to be the other way around, since the college has the greater influence in molding men. Education's method of raising scholarship standards, by denying its privileges to the scholastically unfit, can succeed also in raising standards of character by denying entrance to the ethically or morally unfit.

Aptitude.—Next to character, aptitude for research is the quality most looked for in research men. The inborn aptitudes needed for research include imagination, analytic power and curiosity. *Imagination*, the ability to visualize situations and foresee possible consequences, is a prime requisite for pioneer research. Without it a man may be useful in the laboratory, but will seldom make discoveries. Then he is likely to be dissatisfied with his accomplishments, especially if he has other abilities that would make him successful in, say, engineering or business. *Analytic power* is the ability to size up a situation. By virtue of it the physicist is a problem-solver. Without it a man draws a wrong conclusion, or none at all, from every experiment. *Curiosity* is the most important research quality, in the opinion of a great pioneer of industrial research, W. R. Whitney. It is the aptitude that motivates research-for-the-fun-of-it. To one endowed with it, Nature is full of interesting problems. If, in addition, he possesses the ability and character needed to make his efforts bring satisfaction, and is paid a salary for doing the thing he likes best to do, he is properly placed.

The university's function in regard to aptitudes is guidance. Although all colleges recognize this, advice and placement should be a much larger part of their responsibility, if correct placement and happiness are as intimately related as I have indicated. By placement I mean finding the right kind of activity rather than getting a job. Few boys know what they want to do. They enter college with the question, "What have you?" The function of broad education is to show them what treasures of activity life holds and to give them opportunity to try these different activities and find which ones give them satisfaction. The time is short and the range of subjects long. Correct placement in four years is possible only under guidance of sympathetic and alert teachers. More is involved than mere loss of time in following false trails; for if a certain line of training has been followed to the degree stage and then is found unsatisfactory, it is difficult to make a change.

I have purposely emphasized activity, rather than mere learning, as a goal in education. Coincident with learning about the treasures of knowledge must go the assessment of one's ability to contribute to them. Our greatest satis-

factions come from accomplishment, which is attainable only in the line of one's greatest aptitude. Should not, then, the greatest emphasis be placed on learning *to do* by experiment—in trying one's wings? The broad college curriculum is ideally adapted to help each man determine what to do, and emphasis on this objective is the first step toward correct placement. When men reach the professional-training stage, a second task is to advise and "screen" them, lest they choose wrongly because of ignorance or false standards, rating salary considerations above aptitude and congeniality. Better screening at this level would be a great service to industry and could save many human tragedies.

Attitudes.—While aptitudes can only be recognized, *attitudes* can be taught and, next to character, are the most important product of education. This is no new doctrine; the Great Book taught that "As a man thinketh in his heart, so is he." Industry is more interested in what a man thinks in his heart—in his attitudes toward his work, subject and fellow men—than in what he knows. If he loves his work, so that days are too short, and passionately desires to know, so that he reads and studies, he will soon outstrip the most erudite pedant. University training at best is too brief to provide more than a small part of the knowledge needed by a research man. The rest must be acquired afterward by continual study. In emphasizing this self-education, I do not mean to minimize the value of formal training. The university must impart in an enduring manner the attitudes that make self-education possible.

Most college teachers and administrators will agree that attainment of this objective falls short of the goal. The criticism does not apply to graduate schools, which, on the whole, do an excellent job of imparting correct attitudes. Their success may be attributed partly to small classes, allowing personal contact with teachers; but mainly, I think, to the influence of fellow graduate students, who, having caught these attitudes from good teachers in the first instance, transmit them from class to class by infection.

Student influence accounts also for the relatively poor attitude toward work and study in most undergraduate schools. College "spirit" is a living thing, transmitted from class to class, through decades and even centuries. Its persistence is one of the great factors in education, for the influence of fellow students is a more potent educational force than books or teachers. It is, on the whole, a healthy influence; but it is not often a scholarly influence. I believe that this attitude, caught from fellow students, spells the principal handicap in research work of the B.S. graduate compared to that of the man who has done graduate work. It is not so much the extra knowledge obtained in graduate school as the changed attitude toward study that enables the one to advance while the other often reaches a ceiling. Could this be changed? Could *desire for knowledge* be attained at the undergraduate level?

Other attitudes—toward work, play, friendship, tolerance—are needed by the research worker and are also products of group influence. Character, too, is molded by this influence. How can this all-powerful group spirit be controlled and molded? One method that works in industrial laboratories is leaven. The beginning must be small—a qualified teacher with two students, then four, then eight, expanding only as fast as new members can be infected with the spirit. Such a group could eventually become strong enough to leaven a whole college. The process is slow, but the stakes are high. It might be worth trying.

Knowledge.—Last and least of these four groups of qualities comes knowledge, but only because of the high valuation of the other three, not because it is unimportant. To obtain new knowledge is the chief aim of research, and the accumulated knowledge of the past is its working capital. Moreover, the pursuit of knowledge furnishes the training ground on which the qualities of character and attitude are forged.

Science faculties today are giving much thought to the question of the kind of knowledge needed for industrial research. For laboratories with long range research programs, the answer is: first and most important, fundamentals—as broad a range as possible. In addition, expertness in some one subject, as part of the training, *but what the subject is matters little.* Thus men trained in physical chemistry have made excellent physicists, becoming experts in, say, high frequency electronics in a few months. Similarly, specialization in nuclear physics is a satisfactory training for industrial physics, provided it is combined with broad fundamental training.

For short range projects, the requirements are different. Here one is hired to work on a specific problem or type of development, with less emphasis on long range usefulness. The requirements then are *training and expert knowledge in this particular field,* although broad fundamental training still is desirable. In preparing for short range research, therefore, the subject should be one for which there is a current demand.

To summarize: Knowledge, valuable as it is, is a less important qualification for industrial research than character, aptitude and attitudes. Aptitude cannot be trained, but can and should be screened by the college more than it is today. Character and attitudes are molded by college influences, for better or worse, and therefore are the responsibilities of the college, just as much as scholarship. That the molding influence is the student body, more than the faculty, makes the problem different from that of scholarship, and to deal with it a different method must be found. Why not devote more effort to this problem?—ALBERT W. HULL, *Science* **101**, 157–160 (1945).

Instructions to Lantern Slide Operators

One demonstration of the following instructions is always sufficient for even the most inexperienced operator: (i) ignore the markings, since they are not standardized; (ii) hold the slide between the eye and screen so that it is as it should appear on the screen; (iii) take the slide by the bottom right- or left-hand *corner* and insert it into the holder by a diving movement, "head-first," so that the top edge rests on the bottom edge of the carrier.—G. PARR, *Rev. Sci. Inst.* **22**, 54 (1945).